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MDCCCVI.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

CONTENTS.

APPENDIX.

ERRATA IN THE MEMOIR ON IMAGINARY QUANTITIES.

Page 26, line 23, *for* 3e. *read* 2de.

—— 32, — 14, *for* $(1 \pm \sqrt{1})$, *read* $(1 \pm \sqrt{-1}$.

—— 40, — 19, *for* $\sqrt{2 \times \sqrt{2}}$, *read* $\sqrt{2} \times \sqrt{2}$.

—— 69, — 15, *for* $+ \frac{(0 \times n \cdot 90^{\circ} \sqrt{-1})}{1 \cdot 2}$, *read* $+ \frac{(0 \times n \cdot 90^{\circ} \sqrt{-1})^2}{1 \cdot 2}$

—— 70, — 19, *for* cercle et, *read* centre à.

—— 76, — 18, *for* $\Big\{ (\text{sin. } a\sqrt{-1} \text{ cos, } a)^{\frac{1}{3}}$, *read* $\Big\{ (\text{sin. } a\sqrt{-1} + \text{cos. } a)^{\frac{1}{3}}$.

—— 82, — 6, *for* $\pm px$, *read* $\pm \sqrt{px}$.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation for the year 1805 to HUMPHRY DAVY, Esq. F.R.S. for his various communications published in the Philosophical Transactions.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture on the Arrangement and mechanical Action of the Muscles of Fishes. By* Anthony Carlisle, *Esq. F.R.S. F.L.S.*

Read November 7, 1805.

It was my intention to have continued my physiological inquiries on the phenomena of muscular motion, by a series of chemical experiments; and to have communicated the result, when duly matured, to the Royal Society. But an unexpected request, made at a late period, for the Lecture of the present year, obliges me to defer those researches, and to limit the investigation of the subject I have chosen.

The application of the motive organs of animals has already furnished examples of general utility by increasing our knowledge of mechanical powers; and the cultivation of this study promises still further improvement.

The muscles of fishes are of a very different construction from those of the other natural classes. The medium in which these animals reside, the form of their bodies, and the

instruments employed for their progressive motion, give them a character peculiarly distinct from the rest of the creation. The frame-work of bones or cartilages, called the Skeleton, is simple; the limbs are not formed for complicated motions, and the proportion of muscular flesh is remarkably large. The muscles of fishes have no tendinous chords, their insertions being always fleshy. There are, however, semi-transparent, pearly tendons placed between the plates of muscles, which give origin to a series of short muscular fibres passing nearly at right angles between the surfaces of the adjoining plates. LEWENHOECK* appears to have overlooked these tendons, and the numerous vessels, which he describes in the interstices of the muscular flakes, I have not been able to discern.

The motion of a round shaped fish, independent of its fins, is simple; and as it is chiefly effected by the lateral flexure of the spine and tail, upon which the great mass of its muscular flesh is employed, whilst the fins are moved by small muscles, and those, from their position, comparatively but of little power, I shall only describe in detail the arrangement and application of those masses, which constitute the principal moving organs.

For this purpose a well known fish, the cod,† has been selected as a standard of comparison for the muscles of other fishes, there being a conspicuous resemblance among them all.

The pairs of fins have been considered as analogous to feet, but they are only employed for the purposes of turning, stopping, altering the position of the fish towards the horizon,

* Phil. Trans. Vol. XXXI. p. 190.

† *Gadus Morhua* of LINNÆUS.

and for keeping the back upwards. The single fins appear to prevent the rolling of the body, whilst the tail is employed to impel it forward.

Each of those fins, which are in pairs, is capable of four motions, *viz.* of flexion and extension, like oars, and of expanding the rays, and closing them.

The extension of the whole fin is performed by a single radiated muscle, which is often supplied with red blood: the antagonist is of a similar character. The great power of the extensor muscle (*Vide* Plate I. *a, a,*) shews how strongly it is required to act when employed to stop suddenly the progressive motion. A series of intervening muscles expand and close the rays.

In the act of extending the fin the interosseal muscles are passive. It is advanced forward edgeways and closed; but during its flexion, the rays are expanded, striking the water with its broadest surface: this action assists the tail in turning the fish. In the effort to stop, these fins are strongly retained at right angles with the body, by the force of the extensor muscles, the rays are expanded, and the effect is assisted by the tail turning laterally with its broadest surface forward.

The single fins, for the expansion and contraction of their rays, are furnished with two sets of muscles; one of which is situated at their roots, and lies oblique; (*bbbbb*) the other, parallel with the spines, to which the rays are articulated (*cc.*) The fin has also a lateral motion, by which it is occasionally drawn out of a straight line; and by the co-operation of these muscles on both sides, it is kept steady whilst the body of the fish is turned oblique in swift motion, or in eddies.

When placed near the tail, the single fins seem also to aid the effect of that instrument by increasing its breadth.

The tail is the principal organ of progressive motion, and its actions are performed by the great mass of lateral muscles. There are a series of short muscles for the purpose of changing the figure of the tail fin, which arise from the spine and *coccyx*, and are attached to the rays immediately beyond their joints: *(dd):* their action is to expand the rays, and by partial contractions to alter the lateral position of the fin. Slender muscles are placed between the several rays, *(ee,)* whose office is to converge them previous to the stroke of the tail.

The muscles situated on the head are those, which act on the *membrana branchiostega*, the under jaw, *os hyoides*, *fauces*, and the globe of the eye.

In order to determine the effect of the fins on the motions of fishes, a number of living dace,* of an equal size, were put into a large vessel of water. The pectoral fins of one of these fishes were cut off, and it was replaced with the others. Its progressive motion was not at all impeded; but the head inclined downwards, and when it attempted to ascend, the effort was accomplished with difficulty.

The pectoral and abdominal fins were then removed from a second fish. It remained at the bottom of the vessel, and could not be made to ascend. Its progressive motion was not perceptibly more slow; but when the tail acted, the body shewed a tendency to roll, and the single fins were widely expanded, as if to counteract this effect.

From a third fish, the single fins were taken off. This

* *Cyprinus leuciscus.*

produced an evident tendency to turn round, and the pectoral fins were kept constantly extended to obviate that motion.

From a fourth fish, the pectoral and abdominal fins were cut off on one side, and it immediately lost the power of keeping the back upwards. The single fins were expanded, but the fish swam obliquely on its side with the remaining pectoral and abdominal fins downwards.

From a fifth fish, all the fins were removed. Its back was kept in a vertical position, whilst at rest, by the expansion of the tail, but it rolled half round at every attempt to move.

From a sixth fish, the tail was cut off close to the body. Its progressive motion was considerably impeded, and the flexions of the spine were much increased during the endeavour to advance: but neither the pectoral nor abdominal fins seemed to be more actively employed.

From a seventh fish, all the fins and the tail were removed. It remained almost without motion, floating near the surface of the water, with its belly upward.

These experiments were repeated on the roach,* the gudgeon,† and the minnow,‡ with similar results.

The muscles of fishes differ materially in their texture from those of other animals: they are apparently more homogeneous, their fibres are not so much fasciculated, but run more parallel to each other, and are always comparatively shorter. They become corrugated at the temperature of 156° of FAHRENHEIT, when their tendinous and ligamentous attachments are dissolved, and their serous juices coagulated. Under those circumstances the muscles lose their transparency, and the lateral cohesion of their fibres is lessened.

* *Cyprinus rutilus.* † *Cyprinus gobio.* ‡ *Cyprinus phoxinus.*

But the mechanical arrangement and physiology of the lateral muscles of the body of fishes constitute my present object. These parts have already been described in a general way by Professor CAMPER, M. VICQ-D-AZYR, and M. CUVIER, to whom I am indebted for much useful information. They have been denominated "*couches musculaires*" by M. VICQ-D-AZYR,* and "*muscles laterals*" by M. CUVIER.† The term used by M. CUVIER seems very appropriate for the general division or class. But, as the flakes are arranged in distinct longitudinal rows, these rows must be considered as orders. And, as "*couches*" appears objectionable, I shall adopt *series* in its stead; distinguishing each by a word referring to its situation in the animal, *viz.* the dorsal, vertebral, abdominal, and ventral series.

These series are composed of thin masses of muscle, or, as they are commonly called, flakes; which for the most part are thicker upon their outward edges, and become wedge-shaped towards their interior attachments. Each series is separated from the next adjoining by a membraneous partition, which is most apparent between the vertebral and abdominal series.

The dorsal series *(ff)* arises from the back of the head. In its course it is terminated on the upper edge by the bones, which support the single fins, and a membranous *septum*: at this part the flakes are thin. Its lower margin is bounded by the vertebral series, where the flakes become gradually thicker. The first flake is composed of longer fibres than the rest, and possesses more red blood. Those succeeding it

* *Mem. étrangers de l'Académ. des Sci. de Paris.* Tom. VII. p. 18. et 223.

† *Leçons d'Anatomie Comparée.* Vol. I. p. 196.

range obliquely backwards. They are all joined together by cellular membrane, and shining fasciæ, which resemble the tendinous expansions in quadrupeds.

Towards the middle of the fish the flakes are thicker, and stand more perpendicular to the surface, becoming oblique and thin as they approach the tail; whilst the intervening fasciæ are most dense at each extremity. This series consists of forty-five flakes, a number corresponding with that of the spinous processes to which they are attached, and which does not vary with the growth of the fish.

The muscular fibres constituting each flake, run nearly at right angles with its anterior and posterior surfaces, and parallel to the length and surface of the fish; except that their posterior extremities incline somewhat inwards.

As the skull affords the ultimate fixed attachment of this series, and its moveable insertions are on the vertebræ, and the tail, it follows, that its combined action is to bend the whole body and tail towards one side; or, if the flakes contract partially, to give it a serpentine motion. To produce these effects all the other series co-operate.

The superior external edges of the flakes of the vertebral series *(gg)* form acute angles with the inferior external edges of those of the dorsal series, the apices of which point towards the tail: the flakes are larger, but their number is the same. The lower margin of this series is bounded by the central membranous partition, which has already been noticed to be more conspicuous than the other longitudinal divisions, and it apparently admits of greater motion.

The abdominal series *(hh)* is composed of flakes similar to the preceding. They range towards the tail, forming an angle

with those of the vertebral series, the apex of which is presented towards the head. They are attached internally to the transverse and inferior spinous processes of the vertebræ. The ribs are placed in the line of the centre partition, and lie between the flakes. This series arises from a bone which borders the opening for the gills, and the pectoral fin, with its scapula and muscles, is situated between its foremost flakes. Wherever this series encloses the viscera, its flakes are shallow, and their thickness internally is not much less than at their external superficies.

Lastly, the flakes of the ventral series *(ii)* form acute angles with the abdominal flakes, the points of which incline to the tail. It is attached anteriorly to the *os hyoides*, and the bones of the lower jaw. In its course it is bounded above by the abdominal series, and below by a membranous *septum*, within which the inferior single fins arise. The flakes, that cover the viscera, are shallow; and they lie more oblique as they approach the tail. Both this, and the last described series, have their muscular fibres arranged according to the length and figure of the fish.

Three large superficial nerves *(kk)* passing longitudinally from the head to the tail, in the course of the membranous partitions, give off fibrils at right angles, which bend inwards between each of the muscular flakes. A larger set of nerves are sent from the *medulla spinalis*, one between each flake, the branches of which seem to enter without ramifying there. Another small nerve passing from the head, and running deep-seated, and close to the dorsal spines, crosses and unites with each of the spinal fibrils, and at the junction a remarkable body appears: it is a loose transparent vesicle, about the

size of a millet-seed, containing a white substance like the carbonate of lime found in the intercostal ganglions of frogs. This vesicle is included within the sheath of the nerve.

The coats of the blood-vessels are of a delicate texture, and easily ruptured. In order, therefore, to secure them from being injured by the violent and sudden actions of the muscles, the principal trunks both of the arteries and veins are inclosed in osseous canals, formed by the bases of the superior and inferior spinous processes; and their first ramifications lie within grooves in the spines. As they pass out to supply the muscles, their branches are immediately subdivided, so that a considerable vessel soon becomes extremely minute.

The rate, at which many fishes move through a medium so dense as water, is very remarkable; their velocity being scarcely surpassed by the flight of the swiftest birds: and although the large proportion of muscles, and their advantageous application, may partly account for the phenomenon, yet the power would be inadequate to the effect, if it were not suddenly enforced; as is evident from the slow progress of eels, and such fishes as are incapable from their length, and flexibility, of giving a sudden lateral stroke.

But the quickness and force of action in the muscles of fishes are counterpoised by the short duration of their powers. Those accustomed to the diversion of angling, are aware how speedily the strength of a fish is exhausted, for if, when hooked, it be kept in constant action, it soon loses even the ability to preserve its balance, and turns upon its side, fatigued and incapable of motion. This has been vulgarly attributed to drowning, in consequence of the mouth being closed upon

the hook; but the same effects take place when the hook is fastened to the side, or tail. This prostration of strength may depend partly on fear, and partly on interrupted respiration, since fishes, when swimming rapidly, keep the *membranæ branchiostegæ* closed, and when nearly exhausted, act violently with their gills.

The shortness of the muscular fibres, and the multiplied ramifications of the blood-vessels, are probably peculiar adaptations for the purpose of gaining velocity of action, which seems to be invariably connected with a very limited duration of it. Such examples form an obvious contrast with the muscular structure of slow-moving animals, and with those partial arrangements where unusual continuance of action is concomitant.

Since my former communications on the subject of cylindrical arteries,* another instance of their supplying slow-moving muscles, which are capable of long continued action, has been pointed out to me by Mr. MACARTNEY. It is in the muscles, which act upon the feet and toes of many birds, and seems to be an adaptation for the long exertion of those muscles while they sleep, and also when they alternately retract one foot under the feathers to preserve it from the effects of cold.

The muscles of the human body, which perform the most sudden actions, have their masses of fibres subdivided by transverse tendons, or are arranged in a penniform direction. The semi-tendinosus, and semi-membranosus of the thigh are thus constructed; the former having its fleshy belly divided by a narrow *fascia*, and the fibres of the latter being ranged

† Phil. Trans. 1800, p. 98.—Also 1804, p. 17.

in a half-penniform manner. The *recti abdominis* are also divided into short masses by transverse tendons, and all these muscles are conjointly employed in the action of leaping.

Perhaps these observations may indicate the reason for that diversity in the lengths of various muscles, which act together; thus, organs of velocity are joined with those of power, and mutually co-operate to produce a simultaneous effect.

Description of Plate I.

The drawing was made from a cod which had been coagulated by heat, in a case of plaister of Paris, the skin being taken away, and an equal portion of the flakes carefully removed from each series, to exhibit their several directions. The subject was reduced to the present size by accurate measurements.

aa, Muscles which extend the pectoral and jugular fins.

bbbbb, Oblique muscles, which erect the rays of the single fins.

cc, Muscles which depress the rays.

dd, Muscles which extend the rays of the tail.

ee, Interosseal muscles, which close the rays.

ff, The dorsal series of muscular flakes.

gg, The vertebral series.

hh, The abdominal series.

ii, The ventral series.

kkk, Three superficial nerves which run longitudinally between the series of flakes.

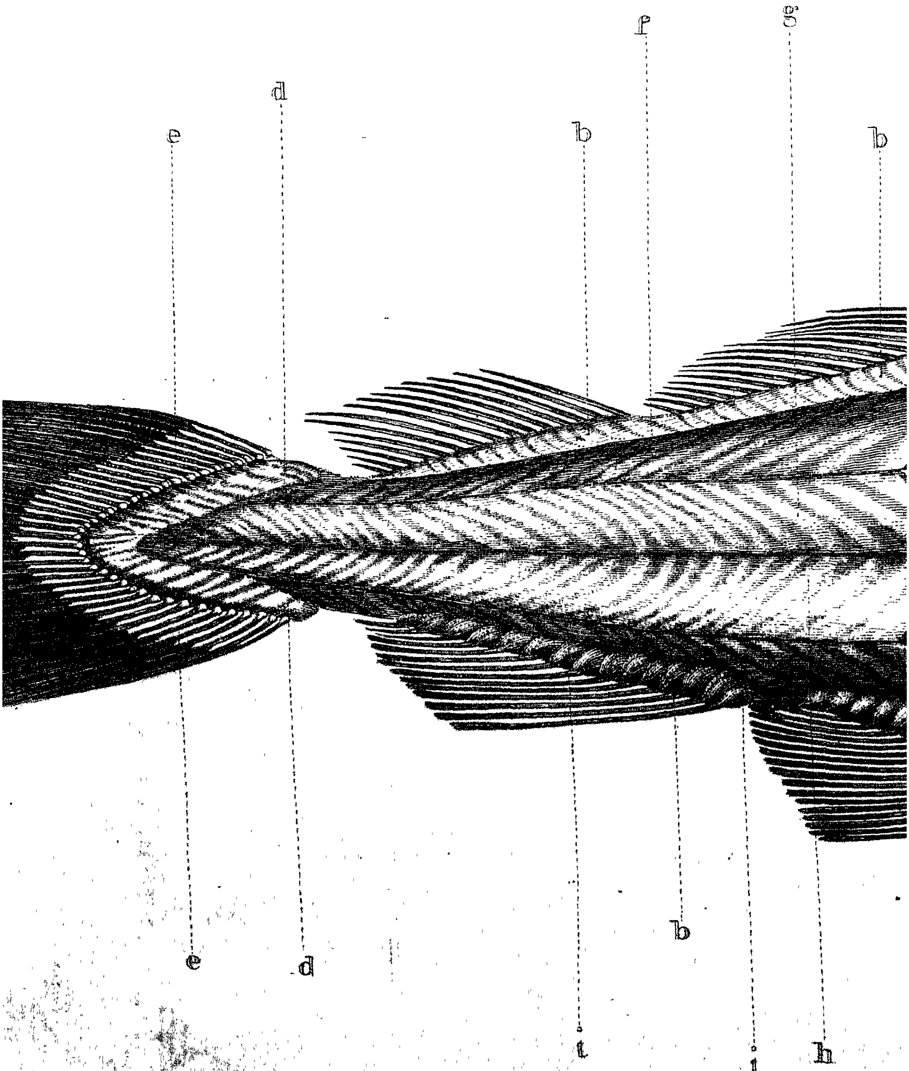
l, Posterior surface of a dorsal flake.

m, Posterior surface of an abdominal flake.

n, Anterior surface of a vertebral flake.

o, Anterior surface of an abdominal flake.

The middle portion of the fish from whence the flakes have been removed, shews the several directions of them, and also their different thicknesses. The spine appears in the chasm.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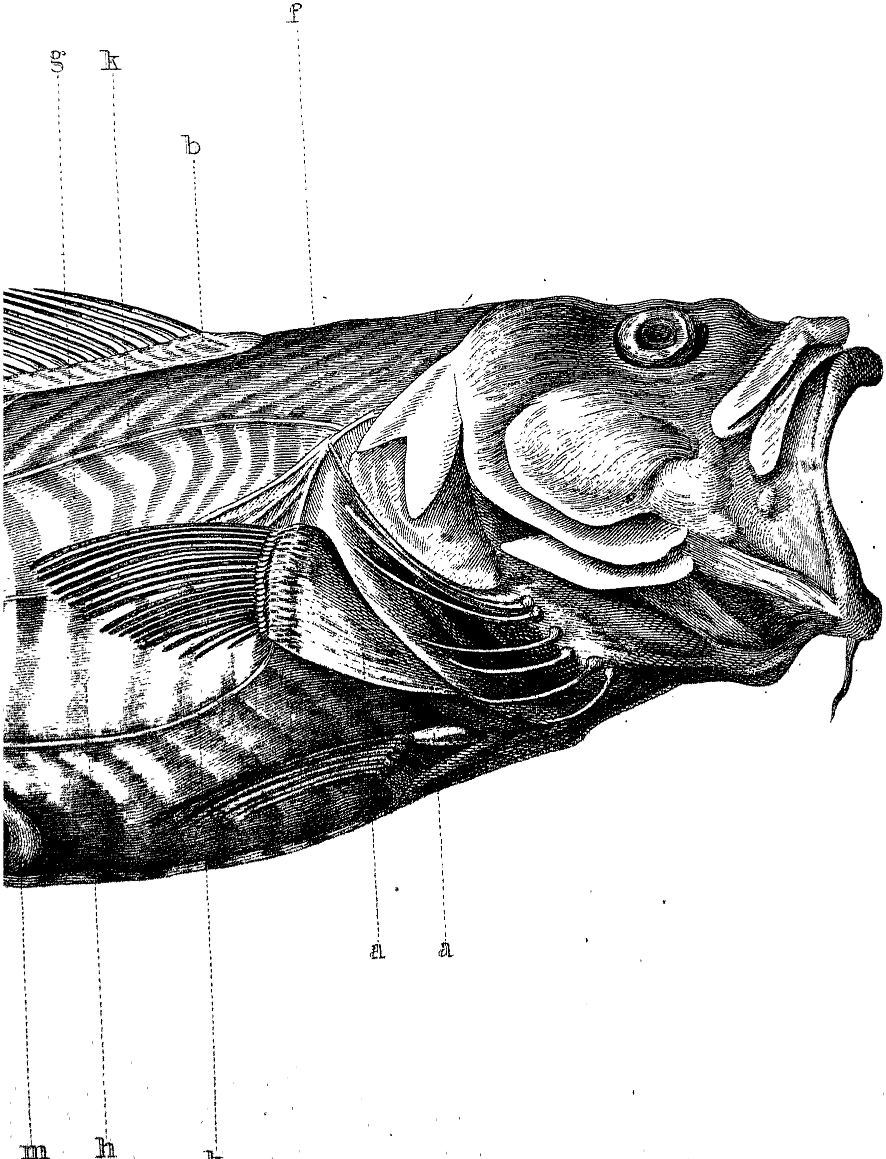

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II. *The Bakerian Lecture on the Force of Percussion. By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read November 14, 1805.

When different bodies move with the same velocity, it is universally agreed that the forces, which they can exert against any obstacle opposed to them, are in proportion to the quantities of matter contained in the bodies respectively. But, when equal bodies move with unequal velocities, the estimation of their forces has been a subject of dispute between different classes of philosophers. Leibnitz and his followers have maintained that the forces of bodies are as the masses multiplied into the *squares* of their velocities, (a multiple to which I shall for conciseness give the name of *impetus*); while those, who are considered as Newtonians, conceive that the forces are in the *simple ratio* of the velocities, and consequently as the *momentum* or *quantitas motus*, a name given by Newton to the multiple of the velocity of a body simply taken into its quantity of matter.

It cannot be expected that at this time any new experiment should be thought of, by which the controversy can be decided, since the most simple experiments that have already been appealed to by either party have received different interpretations from their opponents, although the facts were admitted.

My object in the present Lecture is to consider which of these opinions respecting the force exerted by moving bodies is most conformable to the usual meaning of that word, and to shew that the explanation given by NEWTON of the third law of motion is in no respect favourable to those who in their view of this question have been called NEWTONIANS.

If bodies were made to act upon each other under the circumstances which I am about to describe, the leading phænomena would occur, which afford the grounds of reasoning on either side.

Let a ball of clay or of any other soft and wholly inelastic substance be suspended at rest, but free to move in any direction with the slightest impulse; and let there be two pegs similar and equal in every respect inserted slightly into its opposite sides. Let there be also two other bodies, A and B, of any magnitude, which are to each other in the proportion of 2 to 1; suspended in such a position, that when perfectly at rest they shall be in contact with the extremities of the opposite pegs without pressing against them. Now if these bodies were made to swing with motions so adapted that in falling from heights in the proportion of 1 to 4 they might strike at the same instant against the pegs opposite to them, the ball of clay would not be moved from its place to either side; nevertheless the peg impelled by the smaller body B, which has the double velocity, would be found to have penetrated twice as far as the peg impelled by A.

It is unnecessary to make the experiment precisely as here stated, since the results are admitted as facts by both parties; but upon these facts they reason differently.

One side observing that the ball of clay remains unmoved,

considers the proof indisputable that the action of the body A is equal to that of B, and that their forces are properly measured by their momenta, which are equal, because their velocities are in the simple inverse ratio of the bodies. Their opponents think it equally proved by the unequal depths to which the pegs have penetrated, that the causes of these effects are unequal, as they find to be the case in their estimation of the forces by the squares of the velocities.

One party is satisfied that equal *momenta* can resist equal pressures during the same *time;* the other party attend to the *spaces* through which the same moving force is exerted, and finding them in the proportion of 2 to 1, are convinced that the *vis viva* of a body in motion is justly estimated by its magnitude and the square of its velocity jointly.

The former conception of a quantity dependent on the continuance of a given *vis motrix* for a certain *time* may have its use, when correctly applied, in certain philosophical considerations; but the latter idea of a quantity resulting from the same force exerted through a determinate *space* is of greater practical utility, as it occurs daily in the usual occupations of men; since any quantity of work performed is always appreciated by the extent of effect resulting from their exertions; for it is well known, that the raising any great weight 40 feet would require 4 times as much labour as would be requisite to raise an equal weight to the height of 10 feet, and that in its slow descent the former would produce 4 times the effect of the latter in continuing the motion of any kind of machine. Moreover, if the weights so raised were suffered to fall freely through the heights that have been ascended by means of 4 and of 1 minute's labour, the velocities acquired

would be in the ratio of 2 to 1, and the squares of the velocities in proportion to the quantities of labour from which they originated, or as 4 to 1; and if the forces acquired by their descent were employed in driving piles, their more sudden effects produced would be found to be in that same ratio.

This species of force has been, first by BERNOUILLI and afterwards by SMEATON, very aptly denominated mechanic force; and when by force of percussion is meant the quantity of mechanic force possessed by a body in motion, to be estimated by its quantity of mechanic effect, I apprehend it cannot be controverted that it is in proportion to the magnitude of the body and to the square of its velocity jointly.

But of this quantity of force NEWTON no where treats, and has accordingly given no definition of it. If, after defining what he meant by the *quantitas acceleratrix*, and *quantitas motrix*, he had had occasion to convey an equally distinct idea of the *quantitas mechanica* resulting from the continued action of any force, he might, not improbably, have proceeded conformably to the definition given by SMEATON, and have added

——quantitas mechanica est mensura proportionalis spatio per quod data vis motrix exercetur;

or, if speaking with reference to the accumulated energy communicated to a body in motion,

——proportionalis quadrato velocitatis quam in dato corpore generat.

But, if we attend to the words of his preface to the first edition of his *Principia*, he evidently had no need of such a definition;

"Nos autem non artibus sed philosophiæ consulentes, deque "potentiis non manualibus sed naturalibus scribentes," &c.

And again, nearly to the same effect in the *Scholium*, which follows the laws of motion, "Cæterum mechanicam tractare "non est hujus instituti."

In the third law of motion he has on the contrary been supposed to speak of this force from an ambiguity in the signification of the words *actio* and *reactio*. By these, however, NEWTON certainly meant a mere *vis motrix* or pressure, as he himself explains them. "Quicquid premit vel trahit alterum, "tantundem ab eo premitur vel trahitur. Si quis lapidem "digito premit, premitur et hujus digitus a lapide," &c. The same meaning is equally evident from his demonstration of the third corollary to the laws, in which he asserts that the *quantitas motûs* of two or more bodies estimated in any given direction is not altered by their action upon each other. The demonstration begins thus:

"Etenim actio eique contraria reactio æquales sunt per legem "tertiam, ideoque per legem secundam æquales in motibus "efficient mutationes versus contrarias partes." Now, if he had considered the third law as implying equality of more than mere moving forces, there could have been no occasion to refer to the second law, with a view thence to deduce the equality of momenta produced.

Some authors however have interpreted the third law differently, and accordingly have expressed a difficulty in comprehending the simple illustration given by NEWTON. When they say that action is equal to reaction, they mean not only that the instantaneous intensity of the moving forces, or pressures opposed to each other, are necessarily equal, but

conceive also a species of accumulated force residing in a moving body, which is capable of resisting pressure during a *time* that is proportional to its momentum or quantitas motus.

If it be of any real utility to give the name of force to this complex idea of *vis motrix* extended through time, as well as that of *momentum* to its effects when unresisted, it would be requisite to distinguish this force always by some such appellation as *momental* force; for it is to be apprehended that for want of this distinction many writers themselves, and it is certain that many readers of diquisitions on this subject have confounded and compared together *vis motrix*, *momentum*, and *vis mechanica*: quantities, that are all of them totally dissimilar, and bear no more comparison to each other, than lines to surfaces, or surfaces to solids.

In practical mechanics, however, it is at least very rarely that the *momentum* of bodies is in any degree an object of consideration: the strength of machinery being in every case to be adapted to the *quantitas motrix*, and the extent and value of the effect to be produced depending upon the *quantitas mechanica* of the force applied, or in other words to the space through which a given vis motrix is exerted.

The comparative velocities given by different quantities of mechanic force to bodies of equal or unequal magnitude have been so distinctly treated of by SMEATON, in a series of most direct experiments,* that it would be a needless waste of time to reconsider them in this place. So also, on the contrary, the quantities of extended mechanic effect producible by bodies moving with different quantities of impetus have been as clearly traced by the same accurate experimentalist.†

But there is one view, in which the comparative forces of

* Phil. Trans. Vol. LXVI. 450. † Vol. LXXII. 337.

impact of different bodies was not examined by SMEATON, and it may be worth while to shew that when the whole energy of a body A is employed without loss in giving velocity to a second body B, the *impetus* which B receives is in all cases equal to that of A, and the force transferred to B, or by it to any third body C, (if also communicated without loss, and duly estimated as a mechanic force,) is always equal to that from which it originated.

As the simplest case of entire transfer, the body A may be supposed to act upon B in a direct line through the medium of a light spring, so contrived that the spring is prevented by a ratchet from returning in the direction towards A, but expands again entirely in the direction towards B, and by that means exerts the whole force which had been wound up by the action of A, in giving motion to B alone. In this case, since the moving force of the spring is the same upon each of the bodies, the accelerating force acting upon B at each point is to the retarding force opposed to A at the corresponding points in the reciprocal ratio of the bodies, and the squares of the velocities produced and destroyed by its action through a given space will consequently be in that same ratio. The momentum, which is in the simple reciprocal ratio of the bodies, might consequently be increased at pleasure by the means proposed, in the subduplicate ratio of the bodies employed; and if momentum were an efficient force capable of reproducing itself, and of overcoming friction in proportion to its estimated magnitude, the additional force acquired by such a means of increase, might be employed for counteracting the usual resistances, and perpetual motion would be easily effected. But since the *impetus* remains unaltered, it is evident

that the utmost which the body B could effect in return would be the reproduction of A's velocity, and restitution of its entire mechanic force neither increased nor diminished, excepting by the necessary imperfection of machinery. The possibility of perpetual motion is consequently inconsistent with those principles which measure the quantity of force by the quantity of its extended effect, or by the square of the velocity which it can produce.

In estimating the utmost effect which one body can produce upon another at rest, the same result is obtained by employing *impetus* as ascensional force, according to HUYGENS; for if the body A were allowed to ascend to the height due to its velocity, and if by any simple mechanical contrivance of a lever or otherwise the body B were to be raised by the descent of A, it is well known that the heights of ascent would be reciprocally as the bodies; and consequently that the *square* of the velocity to be acquired by free descent of B would be in that ratio, and the quantity of mechanic force would be preserved as before unaltered.

It may be of use also to consider another application of the same energy, and to shew more generally that the same quantity of total effect would be the consequence not only of direct action of bodies upon each other, but also of their indirect action through the medium of any mechanical advantage or disadvantage; although the time of action might by that means be increased or decreased in any desired proportion. For instance, if the body supposed to be in motion were to act by means of a lever upon a spring placed at a certain distance from the centre of motion, the retarding force opposed to it would be inversely as the distance of the body

from the centre; and since the space through which the body would move to lose its whole velocity would be reciprocally as the retarding force, the angular motion of the lever and space through which the spring must bend, would be the same, at whatever point of the lever the body acted. And conversely, the reaction of the spring upon any other body B, would in all positions communicate to it the same velocity.

It may be remarked, however, that the times in which these total effects are produced may be varied at pleasure in proportion to the distances at which the bodies are placed from the centre of motion; and it should not pass unobserved that, although the intensity of any *vis motrix* is increased by being placed at what is called a mechanical advantage, yet on the contrary, any quantity of mechanic force is not liable to either increase or diminution by any such variation in the mode of its application.

Since we can by means of any mechanic force consisting of a *vis motrix* exerted through a given *space*, give motion to a body for the purpose of employing its *impetus* for the production of any sudden effect, or can, on the contrary, occasion a moving body to ascend, and thus resolve its *impetus* into a moving force ready to exert itself through a determinate space of descent, and capable of producing precisely the same quantity of mechanic effect as before, the force depending on *impetus* may justly be said to be of the same kind as any other mechanic force, and they may be strictly compared as to quantity.

In this manner we may even compare the force of a body in motion to the same kind of force contained in a given quantity of gunpowder, and may say that we have the same

quantity of mechanic force at command whether we have 1lb. of powder, which by its expansion could give to 1 ton weight a velocity sufficient to raise it through 40 feet, or the weight actually raised to that height and ready to be let down gradually, or the same weight possessing its original velocity to be employed in any sudden exertion.

By making use of the same measure as in the former cases, a distinct expression is likewise obtained for the quantity of mechanic force given to a steam-engine by any quantity of coals; and we are enabled to make a comparison of its effect with the quantity of work that one or more horses may have performed in a day, each being expressed by the space through which a given moving force is exerted. In the case of animal exertion however, considerable uncertainty always prevails in consequence of the unequal powers of animals of the same species, and varying vigour of the same animal. The information which I have received in reply to inquiries respecting the weights raised in one hour by horses in different situations, has varied as far as from 6 to 15 tons to the height of 100 feet. But although the rate at which mechanic force is generated may vary, any quantity of work executed is the same, in whatever time it may have been performed.

In short, whether we are considering the sources of extended exertion or of accumulated energy, whether we compare the accumulated forces themselves by their gradual or by their sudden effects, the idea of mechanic force in practice is always the same, and is proportional to the *space* through which any moving force is exerted or overcome, or to the *square* of the velocity of a body in which such force is accumulated.

III. *Mémoire sur les Quantités imaginaires. Par M.* Buée. *Communicated by* William Morgan, *Esq. F. R. S.*

Read June 20, 1805.

Des Signes + *et* —.

1. CES signes ont des significations opposées.

Considérés comme signes *d'opérations arithmétiques*, + et — sont les signes, l'un de l'*addition*, l'autre de la *soustraction*.

Considérés comme signes d'*opérations géométriques*, ils indiquent des *directions opposées*. Si l'un, par exemple, signifie qu'une ligne doit être tirée de gauche à droite, l'autre signifie qu'elle doit être tirée de droite à gauche.

2. Remarque. Lorsqu'on décrit une ligne d'une longueur déterminée, dans une direction déterminée, on fait deux choses: 1°. on donne à cette ligne sa *longueur*; 2°. on lui donne sa *direction*. La première de ces opérations est purement *arithmétique*. La seconde est purement *géométrique*. Dans la première on fait abstraction de la direction. Dans la seconde on fait abstraction de la longueur. Lors donc qu'on réunit ces deux opérations, on fait réellement une opération *arithmético-géométrique*. Ainsi, lorsque je parlerai d'*opérations géométriques*, je n'aurai en vue que les *directions* des lignes, abstraction faite de leurs longueurs. Revenons aux signes + et —.

3. En général lorsque + et — ne signifient pas simple-

ment, l'un l'addition, et l'autre la soustraction, pour savoir ce que — signifie devant une lettre, il faut savoir ce que signifieroit + devant cette même lettre, et prendre pour — la signification opposée.

Si, par exemple, $+t$ signifie un *temps passé*, $-t$ signifie un *temps futur* égal. Si $+p$ désigne une *propriété*, $-p$ désigne une *dette* de même valeur, &c.

4. Il est important de remarquer ici que, lorsqu'une chose désignée par $+l$, une ligne, par exemple, change de situation dans le courant d'une opération arithmético-géométrique, et qu'en conséquence cette ligne a successivement plusieurs situations (qui toutes sont désignées par $+l$) il ne suffit pas, pour connoître la situation désignée par $-l$, de connoître une de celles qu'on désigne par $+l$; il faut encore savoir à laquelle chaque $-l$ est opposée.

5. Ce détail nous mène à la conséquence suivante.

Chacun des signes + et — a deux significations tout-à-fait différentes.

1°. Mis devant une quantité q, ils peuvent désigner, comme je l'ai dit, deux *opérations arithmétiques* opposées dont cette quantité est le sujet.

2°. Devant cette même quantité, ils peuvent désigner deux *qualités* opposées ayant pour sujet les *unités* dont cette quantité est composée.

6. Dans l'algèbre ordinaire, c'est-à-dire, dans l'algèbre considérée comme *arithmétique universelle*, où l'on fait abstraction de toute espèce de qualité, les signes + et — ne peuvent avoir que la première de ces significations.

Par conséquent, dans cette algèbre où tout est abstrait, une quantité isolée peut bien porter le signe + qui, dans ce cas,

n'ajoute rien à l'idée de cette quantité ; mais elle ne peut pas porter le signe — (c'est un des principes fondamentaux de Mr. CARNOT, dans son excellent traité intitulé : *Géométrie de Position.*) En effet cette quantité etant supposée isolée, si on l'ajoute, ce ne peut être qu'à zéro ; si on la soustrait, ce ne peut être que de zéro. Le premier est possible ; mais le second est absurde. C'est aussi ce que Mr. FREND a bien vu, dans ses élémens d'algèbre.

Par conséquent, toutes les fois qu'on a pour résultat d'une opération une quantité précédée du signe —, il faut, pour que ce résultat ait un sens, y considérer quelque qualité. Alors l'algèbre ne doit plus être regardée simplement comme une *arithmétique universelle*, mais comme une *langue mathématique.*

7. Première remarque. Lorsqu'on a dit* que les signes + et — indiquoient deux sens opposés, on avoit en vue la seconde des significations que j'ai données à ces signes (No. 5) ; car être susceptible d'un sens est une qualité.

De même, lorsqu'on a dit† qu'une quantité négative étoit plus petite que zéro, on avoit encore en vue cette seconde signification ; car ce n'est pas la *quantité* qui est plus petite que *zéro ;* c'est la *qualité* qui est inférieure à la *nullité.* Par exemple, si mes dettes excèdent mes propriétés, je suis plus pauvre que si je n'avois ni propriétés ni dettes.

8. Seconde remarque. Les deux significations de + et — ne peuvent pas avoir lieu en même temps, relativement au même + ou au même — ; car ce seroit faire signifier en même temps, au même signe, une *abstraction* et une *non-abstraction* de toute *qualité.* Mais 1°. ces deux significations

* *Voyéz les Opuscules mathématiques de* D'ALEMBERT, Tome VIII. p. 270.

† EULER, *Introductio in Analysin Infinitorum,* T. II. p. 4, No. 3.

peuvent avoir lieu *en même* temps pour deux + ou deux − différens. 2°. Elles peuvent aussi avoir lieu pour le même + ou le même −, *en deux différens temps.*

Le premier cas arrive dans ce qu'on appelle *la multiplication des signes.* Le second arrive dans la solution d'un probléme. On sait en effet que, pour résoudre algébriquement un probléme, il faut d'abord traduire la question en langage algébrique ; ensuite traduire les formules du langage algébrique en d'autres formules du même langage ; enfin traduire celles-ci dans les opérations qu'elles signifient. Lorsqu'on traduit une question (dont l'objet n'est pas quelque nombre abstrait) en langue algébrique, c'est la seconde signification (No. 5) qu'on doit attribuer aux signes + ou −. Dans la seconde traduction, c'est la première signification. Dans la 3e. c'est la première ou la 2de. ou toutes les deux à la fois (No. 2).

Il arrive dans toutes les solutions de problémes par l'algèbre ce qui arrive dans les plus simples règles de trois. Lorsqu'on traduit la condition de la question en une proportion géométrique, on pose les termes de cette proportion comme des nombres *concrets*, c'est-à-dire, comme des nombres d'unités auxquelles des qualités sont attachées. Lorsqu'on opère une multiplication et une division sur les termes de cette proportion (c'est-à-dire, lorsqu'on fait la 3e. des traductions dont j'ai parlé) on regarde ces termes comme des nombres *abstraits.* Lorsqu'enfin on traduit en langue vulgaire le résultat de ces opérations, on regarde ce resultat comme un nombre *concret.*

9. Troisième remarque. Selon la seconde signification donnée (No. 5) aux signes + et −, ils désignent deux *qualités* opposées ayant pour sujets les *unités* dont une quantité

est composée. Or comme une qualité ne peut être séparée de son sujet, les signes $+$ et $-$ ne peuvent être séparés de leurs unités. Dans la langue algébrique, ces unités sont des *substantifs*, et les signes $+$ et $-$, des *adjectifs*. Par conséquent $+q$ et $-q$ tiennent toujours lieu de $+1 \, . \, q$ et $-1 \, . \, q$, c'est-à-dire, de l'unité $+1$ ou -1 (ayant une qualité quelconque) prise q fois. Cette expression (q fois) marque que q est pris pour un nombre abstrait. De même, si l et l' désignent des lignes, $+l \times l'$ et $-l \times l'$ tiennent lieu de $+1^2 . \, l \times l'$ et $-1^2 . \, l \times l'$, 1^2 etant une surface carrée et $l \times l'$ un nombre abstrait. Si l ou l' désignoit une surface, alors $+l \times l'$ et $-l \times l'$ (qui auroient chacun trois dimensions, savoir, les deux dimensions de la surface et la dimension de la ligne) tiendroient lieu de $+1^3 \times ll'$ et $-1^3 \times ll'$.

Il en seroit de même de toute autre signification de l et de l'. On voit par là toute l'étendue de la signification des adjectifs $+$ et $-$ unis à leur substantif 1.

Du Signe $\sqrt{-1}$.

10. Je mets en titre, *Du signe* $\sqrt{-1}$, et non, *De la quantité ou De l'unité imaginaire* $\sqrt{-1}$; parceque $\sqrt{-1}$ est un signe particulier joint à l'unité réelle 1, et non une quantité particulière. C'est un nouvel adjectif joint au substantif ordinaire 1, et non un nouveau substantif.

Mais que veut dire ce signe ? Il n'indique ni une addition, ni une soustraction, ni une suppression, ni une opposition par rapport aux signes $+$ et $-$. Une quantité accompagnée de $\sqrt{-1}$ n'est ni additive, ni soustractive, ni égale à zéro. La

qualité marquée par $\sqrt{-1}$ n'est opposée ni à celle qu'indique $+$, ni à celle qui est désignée par $-$. Qu'est-elle donc?

Pour le découvrir, supposons trois lignes égales AB, AC, AD, (Plate II. Fig. 1.) qui partent toutes du point A. Si je désigne la ligne AB par $+1$, la ligne AC sera -1, et la ligne AD, qui est une moyenne proportionelle entre AB et AC, sera nécessairement $\sqrt{-1^2}$, ou plus simplement, $\sqrt{-1}$. Ainsi $\sqrt{-1}$ est le signe de la PERPENDICULARITE', dont la propriété caractéristique est, *que tous les points de la perpendiculaire sont également éloignés de points placés à egales distances, de part et d'autre de son pié.* Le signe $\sqrt{-1}$ exprime tout cela, et il est le seul qui l'exprime.

Ce signe mis devant a (*a* signifiant une ligne ou une surface) *veut donc dire: qu'il faut donner à* a *une situation perpendiculaire à celle qu'on lui donneroit, si l'on avoit simplement* $+$ a *ou* $-$ a.

11. Voici une autre manière de parvenir au même résultat.

Soient AB, AD (Fig. 2.) deux côtés contigus du carré ABCD. Supposons AB $= \pm 1$, et par conséquent AD $= \pm 1$, et mettons en A le point de départ de la description des lignes AB et AD, ensorte que AB et AD portent le même signe $+$ ou $-$, et que le carré ABCD soit positif.

Maintenant faisons faire à ce carré ABCD un quart de révolution autour du point A pris comme centre. Après ce mouvement, le point B sera en B', le point C en C', et le point D en D'. Chacune des lignes AB, BC, CD, DA, prendra une situation perpendiculaire à celle qu'elle avoit, et, au lieu du carré ABCD, on aura le carré AB'C'D'. Or A étant le point de départ, il est clair que, si le carré ABCD est positif, le carré AB'C'D' doit être négatif, et *vice versâ.* Par

conséquent si ABCD $= + 1^2$ dont le côté AB ou BC ou CD ou DA est $= \pm 1$, on a AB′C′D′ $= - 1^2$ dont le côté AB′ perpendiculaire à AB, ou B′C′ perpendiculaire à BC, ou C′D′ perpendiculaire à CD, ou D′A perpendiculaire à DA est $= \pm \sqrt{-1}$. On voit donc que, si l'on donne à tous les côtés d'un carré des positions perpendiculaires à celles qu'ils ont, *sans cependant changer leurs positions respectives et en faisant le plus petit mouvement possible* (c'est-à-dire, en n'ajoutant pas le mouvement de translation à celui de rotation) on obtient le même résultat qu'en joignant le signe $\sqrt{-1}$ au signe de ces côtés.

12. $\sqrt{-1}$ n'est donc pas le signe d'une opération *arithmétique*, ni d'une opération *arithmético-géométrique* (No. 2), mais d'une opération purement géométrique. C'est un signe de perpendicularité. C'est un signe purement descriptif. J'appelle signe *purement descriptif* un signe qui indique la *direction* d'une ligne, abstraction faite de sa *longueur*. Ainsi les mots *purement descriptif* ont la même signification que les mots *purement géométrique* (No. 2).

13. Il faut distinguer la perpendicularité indiquée par ce signe de celles qu'indiquent les signes *sin.* et *cos.* Ces derniers signes ne peuvent pas indiquer la perpendicularité l'un sans l'autre, et même si l'un et l'autre ne sont pas attachés à la même quantité. Ainsi *sin. a* et *cos. a* indiquent bien la perpendicularité de l'un à l'autre; mais *sin. a* et *cos. b* ne l'indiquent pas. $a\sqrt{-1}$, au contraire, indique relativement à a une situation perpendiculaire à celles de $+a$ et de $-a$.

Sin. et cos. sont des signes artificiels. $\sqrt{-1}$ est un signe

naturel, puisqu'il est une conséquence nécessaire des signes $+$ et $-$ considérés comme signes de direction.

14. La *perpendicularité* indiquée par le signe $\sqrt{-1}$ est une *qualité.* Par conséquent une quantité accompagnée de ce signe n'est pas une quantité abstraite, parceque ses unités ne sont pas des unités abstraites,

15. Non seulemeut l'unité affectée du signe $\sqrt{-1}$ n'est pas une unité abstraite, mais elle peut être regardée comme une nouvelle indéterminée introduite par ce signe. En effet, ce signe indique la perpendicularité, mais il n'indique que cela. Il n'indique pas le point de départ de la perpendiculaire. Si donc ce point de départ n'est pas déterminé d'ailleurs, ce signe le laisse indéterminé. Ainsi tandis que la *longueur* de la ligne perpendiculaire est *constante*, sa *manière d'être perpendiculaire* est *variable.* De plus, soit AD (Fig. 1.) l'unité à laquelle le signe $\sqrt{-1}$ est attaché. Elle peut être aussi bien AD′, AD″, ou tout autre rayon du cercle DD′D″; puisque ce cercle etant supposé perpendiculaire au plan de ce papier, tous ses rayons sont perpendiculaires à la ligne CB qui est sur le plan de ce papier.

16. Quoique la *perpendicularité* soit *proprement* la seule qualité indiquée par le signe $\sqrt{-1}$, on peut lui faire signifier, *au figuré*, une qualité toute différente, pourvû qu'on puisse raisonner sur cette qualité, comme on raisonneroit sur la perpendicularité même. Par exemple, si $+s$ représente une somme *possédée*, et $-s$ la même somme *due*, je dis que $s\sqrt{-1}$ peut représenter la même somme *ni possédée ni due*, parcequ'on peut raisonner sur cette dernière somme relativement

aux deux autres, comme sur la ligne AD (Fig. 1.) relativement aux lignes AB, AC.

En effet, de même qu'un point quelconque de la ligne AD est également distant des points de la ligne CD qui se trouvent au même éloignement du point A, de même une partie quelconque de la somme qui n'est *ni possedée ni due* est dans une égale situation relativement aux parties égales de la somme *possédée* et de la somme *due*. La *possession active* étant donc exprimée par $+$, et la *dette ou possession passive*, par $-$, la *négation*, non pas de la somme, mais de sa *possession soit active soit passive*, peut toujours être exprimée par $\sqrt{-1}$.

17. On peut, d'après cette idée, résoudre facilement la question suivante.

Problême I.

Un homme possède un nombre n de livres. Il y a de plus un nombre n' de livres qui est par rapport à lui, ou une *propriété*, ou une *dette*, ou une *somme ni possedée ni due*. Si l'on ajoute ensemble les deux nombres n et n', on aura une somme x. Si l'on soustrait n' de n, on aura un différence y. Cette somme et cette différence sont telles que $x+y=a$, et $xy=b$ (b étant une somme *possedée* ou *due* ou *ni l'un ni l'autre*, et a une *propriété*). On demande 1°. les valeurs des nombres n et n'; 2°. si n' est *une propriété ou une dette ou une somme ni possédée ni due?*

D'après l'énoncé de cette question, nous avons les quatre équations suivantes: $x+y=a$; $xy=b$; $n+n'=x$; $n-n'=y$. Ces quatre équations donnent: - - - - -

$x=\frac{a\pm\sqrt{a^2-4b}}{2}$; $y=\frac{a\mp\sqrt{a^2-4b}}{2}$; $n=\frac{a}{2}$; et $n'=\frac{\pm\sqrt{a^2-4b}}{2}$.

Si $4b > a^2$, b étant positif, n' est une somme *ni possédée ni due.*

Si $4b < a^2$, ou si b est négatif, n' est une somme *ou possédée ou due.* Dans ce cas elle augmente ou diminue la somme désignée par n.

Si b porte le signe $\sqrt{-1}$, alors n' est en partie *possedée ou due* et en partie *ni possedée ni due.*

18. Il se présente ici une objection qui d'abord paroît insurmontable. Pour exposer cette objection de la manière la plus simple, substituons des chiffres aux lettres a et b. Faisons $a=2$ et $b=2$. Nous aurons $x=1\pm\sqrt{-1}$; $y=1\mp\sqrt{-1}$; $n=1$; et $n'=\sqrt{-1}=$ la valeur 1 qui n'est ni possedée ni due; $x+y=\overline{1\pm\sqrt{-1}}+\overline{1\mp\sqrt{-1}}=2$; $xy=(1\pm\sqrt{1})\ (1\mp\sqrt{-1})=1^2-(\sqrt{-1})^2=1^2-(-1)=2$. Ce resultat répond à la question; mais on peut croire qu'il n'y répond que d'après une supposition absurde, savoir, que $\sqrt{-1}$, qui n'est ni une propriété ni une dette, étant multiplié par $-\sqrt{-1}$, qui n'est pareillement ni une propriété ni une dette, donne $+1$ qui est une propriété. Comment suffit-il, pour me faire acquérir une propriété, de multiplier une somme qui m'est étrangère par une somme qui m'est également étrangère?

Cette objection est semblable à celle-ci: comment la dette -1 multipliée par la dette -1 peut-elle produire la propriété $+1$? Ce qui fait la force de cette objection tient à ce que, en la proposant, on n'analyse pas exactement ce qui se passe dans l'opération appellée *multiplication.* En effet, si l'on attache aux mots leur signification ordinaire, cette expression, *multiplier une dette par une dette*, ne présente aucun sens

intelligible; car, multiplier, par exemple, 3 par 4, signifie en général, prendre 3, 4 *fois;* de sorte que *multiplier la dette* 3 *par la dette* 4 ne pourroit signifier que, prendre la dette 3 *une dette* 4 *de fois*, ce qui est un galimathias. Cette expression -1×-1 ne doit donc pas être traduite par celle-ci: *la dette* -1 *multipliée par la dette* -1. Voyons comment elle doit être traduite.

Le multiplicateur -1 présente deux idées, savoir, l'idée de l'unité 1 et l'idée exprimée par le signe $-$. Ce dernier signe représente une opération, de sorte que le double signe $\times -$ en représente deux. Cela posé, il est facile de concevoir que -3×-4 signifie: -3 *pris* 4 *fois avec un signe contraire* à celui que donneroit $-3 \times +4$ ou $-3 \cdot 4$ (No. 3.) Les mots: *pris* 4 *fois*, sont la traduction de $\times 4$, et les mots: *avec un signe contraire*, &c. sont celle de $-$. Maintenant si l'on applique ce qui vient d'être dit au quadruple signe $\sqrt{-1} \times -\sqrt{-1}$, on verra que les mots suivans de l'objection: $\sqrt{-1}$, *qui n'est ni une propriété ni une dette, étant multiplié par* $-\sqrt{-1}$ *qui n'est pareillement ni une propriété ni une dette*, que ces mots, dis-je, sont une fausse traduction de $\sqrt{-1} \times -\sqrt{-1}$, et que la vraie traduction de ce signe composé est celle-ci: *La quantité concrète* $\sqrt{-1}$ *prise une fois, dans un sens également éloigné des sens que présenteroient* $\sqrt{-1} \times -(+1)$ et $\sqrt{-1} \times -(-1)$ (Nos. 10 et 16).

Cette explication fait évanouir l'objection. En effet cette objection tombe sur la manière dont la solution du problême remplit la condition exprimée par $xy = b$. xy signifie x *multiplié par* y. Cette expression: *multiplié par* y, doit être prise dans un sens intelligible. Or l'explication que je viens de

donner du signe complexe $\sqrt{-1} \times -\sqrt{-1}$ est (si je ne me trompe) parfaitement intelligible ; elle est même la seule qui puisse l'être ; elle n'offre d'ailleurs aucune absurdité, et conduit à la solution complette du problême proposé. Donc &c.

19. La solution de la question précédente me paroît impossible par l'algèbre ordinaire qui n'attribue aucune signification intelligible au signe $\sqrt{-1}$.

20. On voit par le détail dans lequel je viens d'entrer que, de cela seul qu'une question conduit à un résultat qui renferme le signe $\sqrt{-1}$, il ne s'ensuit pas qu'elle soit absurde.

Il ne s'ensuit pas non plus qu'elle ne le soit pas.

Pour aider à reconnoître ce qui en est, dans les différens cas, posons quelques principes.

21. 1°. Dans les questions purement arithmétiques, le signe $\sqrt{-1}$ devant indiquer une opération arithmétique, et ne le pouvant pas, indique une opération absurde.

22. 2°. $\sqrt{-1}$ indique une *qualité moyenne* entre deux qualités opposées dont l'une est exprimée par $+$ et l'autre par $-$. *Ces qualités doivent être indépendantes de la quantité.* Lors donc que la qualité moyenne n'est pas indépendante de la quantité, elle ne peut être exprimée par $\sqrt{-1}$; car $\sqrt{-1}$ ne peut exprimer qu'une qualité indépendante de toute quantité, c'est-à-dire, une qualité qui reste la même, quoique la quantité varie. Si, par exemple, j'exprime un temps *futur* par $+t$, et un temps *passé* par $-t$, $t\sqrt{-1}$ ne peut rien signifier, parceque le *présent* qui est la qualité moyenne entre le *futur* et le *passé*, n'est qu'un instant indivisible et qu'il n'a d'autre expression que o.

23. Si cependant on entend (car le langage ordinaire est

si vague qu'il est peu d'expressions qu'on ne puisse entendre de plusieurs manières) par *present*, un certain espace de temps, comme, *ce jour-ci*, *le mois présent*, *la présente année*, *le siècle present*, alors $t\sqrt{-1}$ peut avoir une signification. Car, par exemple, soit AB (Fig. 3.) le mois passé $-t$, EF le mois prochain $+t$, BC représentera la première moitié $\frac{-t\sqrt{-1}}{2}$ et DE la seconde moitié $\frac{+t\sqrt{-1}}{2}$ du mois présent, de sorte que l'expression du mois présent entier sera $\frac{-t\sqrt{-1}}{2}+\frac{t\sqrt{-1}}{2}=0$, (voyez le No. suivant). Or o (qui, comme on le verra bientôt, a deux significations) est la veritable expression du présent. De plus, comme les lignes BC, DE, sont égales et perpendiculaires aux lignes AB, EF, on voit que les parties des deux moitiés du mois *présent* qui sont également eloignées de son milieu CD, le sont pareillement, l'une de l'extrémité A du mois *passé*, et l'autre de l'extrémité F du mois *prochain*.

24. Il faut cependant observer que, cette espèce de *présent* ayant des limites constantes, si la valeur de $\pm t\sqrt{-1}$ donnoit des limites différentes de celles qui seroient supposées tacitement, alors il y auroit une contradiction entre cette valeur de $\pm\sqrt{-1}$ et cette supposition tacite. Par conséquent la solution donnée par cette valeur seroit absurde.

25. N^a. On trouvera peut-être une espèce de paralogisme dans l'équation $-\frac{t\sqrt{-1}}{2}+\frac{t\sqrt{-1}}{2}=0$, par laquelle je fais l'éspace d'un mois égal à zéro. Mais on observera 1°. que cette équation ressemble à la phrase d'un homme qui, après s'être égaré se retrouve au point dont il vouloit s'éloigner, et dit: " Je ne suis pas plus avancé, après tant de chemin, que " si je n'en avois fait aucun ;" car le *temps* est pour l'esprit ce

que l'*espace* est pour le corps. On observera 2°. que $\frac{-t\sqrt{-1}}{2}$ $+\frac{t\sqrt{-1}}{2}$ n'est qu'un signe, aussi bien que o. Ce ne sont pas les choses que j'égalise, mais les signes qui présentent ces choses sous un point de vue particulier. Je les égalise, parceque, dans l'exemple actuel, je puis raisonner sur la chose que me présente le double signe $+\sqrt{-1} \; -\sqrt{-1}$, comme sur celle que le signe o me présente, et que l'un et l'autre de ces signes me conduisent aux mêmes conséquences. Cette équation n'est pas réelle. Elle n'est qu'artificielle, comme tout l'est dans l'algèbre. Elle veut dire ceci: *un mois dont on fait abstraction est (relativement aux conséquences) égal à un mois qui n'existe pas.* Dans $\left(\frac{-t\sqrt{-1}+t\sqrt{-1}}{2}\right)$, c'est *la qualité de passé ou de futur* qui est zéro; dans o, c'est *la quantité de passé et de futur* qui l'ést.

26. Le signe o a deux significations. On peut en effet le considérer sous un point de vue *arithmétique* et sous un point de vue *descriptif.* Sous le premier, o signifie *quantité nulle.* Sous le second, il signifie une description telle que la *distance* entre le premier et le dernier point est nulle. (Voyez la Figure 3).

27. Cet exemple fait voir, si je ne me trompe, que les signes $+\sqrt{-1}$ et $-\sqrt{-1}$ peuvent avoir une signification toutes les fois que les qualités représentées par les signes opposés $+$ et $-$ sont telles qu'une unité peut avoir l'*une* ou l'*autre* ou *ni l'une ni* l'autre.

De cette manière les signes $+$ et $-$ peuvent, même comme signes d'addition et de soustraction, admettre au milieu d'eux le signe $\sqrt{-1}$. Alors $+q$ signifie que la quantité q a la

qualité d'être *additive;* $-q$, qu'elle est *soustractive;* $q\sqrt{-1}$, qu'elle n'est *ni additive ni soustractive;* que, par conséquent, $q\sqrt{-1}$ est étranger à l'équation qui contient $+q$ ou $-q$.

N^a^. Il faut bien remarquer ici qu'*être étranger* ne signifie pas *être nul*, mais *être regardé comme nul*. Dans l'exemple présent, *être étranger* signifie : *ni* additif *ni* soustractif. *Etre nul* signifie : additif *et* soustractif *en même temps*.

28. 3°. Lorsque $\sqrt{-1}$ signifie une *qualité* de la chose prise pour unité, examinons ce que la présence de ce signe indique, dans le cas où la question demande qu'on fasse abstraction de cette qualité et qu'en conséquence on ne fasse pas usage de son signe. Quand on fait une abstraction, comme par exemple, quand, en considérant la *longueur* d'une ligne, on fait abstraction de sa *largeur*, il faut que tous les raisonnemens qu'on fait sur cette ligne soient indépendans de cette *largeur*. Il faut par conséquent que le signe de cette largeur ne paroisse pas, ou qu'il soit accompagné, non du signe $\pm$, mais des signes séparés $+$ et $-$ dont l'un puisse détruire l'effet de l'autre. Si $\sqrt{-1}$ exprime cette *largeur* et qu'il paroisse dans le résultat de ces raisonnemens, sans pouvoir être détruit autrement que par une nouvelle équation qui contredise les équations établies, il indique quelque contradiction dans ces raisonnemens. Il indique donc alors une absurdité. Lors donc qu'on trouve ce signe dans un résultat, il faut voir si la question éxigeoit qu'on fît abstraction de la qualité indiquée par ce signe. Si elle l'exigeoit, la question étoit impossible. Sinon, elle ne l'étoit pas.

29. Si elle exigeoit qu'on fît cette abstraction, cette abstraction étoit une condition. L'expression de cette condition devoit être une équation. Cette équation nouvelle supposoit

une nouvelle indéterminée pour que le problême ne fût pas plus que déterminé.* Or (No. 14.) l'unité renfermée dans le signe $\sqrt{-1}$ pouvant être regardée comme un nouvelle indéterminée introduite par ce signe, et son indétermination étant relative non à la *quantité*, mais à la *qualité*, la seule manière d'exprimer qu'on faisoit abstraction de cette qualité, étoit d'égaler à zéro tous les termes multipliés, par $\sqrt{-1}$. Si donc le résultat donne la somme de ces termes comme n'étant pas $=0$, il est en contradiction avec les conditions de la question.

30. 4°. Mais *toutes les fois qu'on peut donner au signe* $\sqrt{-1}$ *une signification compatible avec les conditions du problême*, c'est-à-dire, lorsque ces conditions n'admettent ni n'excluent cette signification, l'indétermination qui accompagne l'unité qu'il renferme corrige le défaut que ce probléme peut sembler avoir d'être plus que déterminé.

31. De tout ce qui vient d'être dit je conclus que partout où l'algébre n'est qu'*arithmétique universelle* et où le signe $\sqrt{-1}$ se trouve mêlé avec les signes $+$ ou $-$ sans pouvoir être supprimé, les quantités qui portent ce signe $\sqrt{-1}$ sont des *quantités imaginaires;* mais que, quand l'algèbre devient une *langue* (et elle le devient dans toute équation, puisque toute équation est une proposition) alors les quantitiés qui portent le signe $\sqrt{-1}$ peuvent être ou n'être pas réelles. Elles le sont lorsque ni la *qualité* figurée par le signe $\sqrt{-1}$, ni la *quantité* affectée de ce signe, ne sont en contradiction avec les conditions de la question.

* *Voyez la Géométrie de Position de* Mr. CARNOT (page 55.)

32. Je crois qu'il est à propos d'ajouter ici une remarque analogue à celle du No. 8.

Dans ce No. je dis que les deux significations de $+$ et $-$ ne peuvent pas avoir lieu en même temps, relativement au même $+$ ou au même $-$. J'en dis autant du signe $\sqrt{-1} \times \sqrt{-1}$. Mais cette remarque a besoin d'être développée.

Principe général. Lorsqu'on a une équation identique, les signes $+$, $-$ et $\sqrt{-1}$ ne peuvent pas avoir une signification dans un de ses membres et un autre signification dans les termes semblables de l'autre membre.

Par exemple, si l'on fait (Fig. 1.) $AB=1$ et $AD=\sqrt{-1}$, on aura $\overline{BD}^2=\overline{AB}^2+\overline{AD}^2=1^2+(\sqrt{-1})^2$. Supposons pour un instant $1^2+(\sqrt{-1})^2=+1-1=0$. On aura $\overline{BD}^2=0$, ce qui est absurde. C'est que le premier membre $1^2+(\sqrt{-1})^2$ ou $\overline{AB}^2+\overline{AD}^2$ représente une *figure* formée de deux carrés égaux tels que les côtés de l'un sont *perpendiculaires* aux côtés correspondans de l'autre (No. 11.); tandis que le second membre $+1-1$ ou 0 signifie la *différence* de deux unités abstraites égales. Ainsi l'équation $\overline{BD}^2=0$ peut être traduite par cette proposition : la *figure* $\overline{BD}^2$ est égale à la *différence de deux unités abstraites*. Cette proposition ne renferme point de contradiction, mais elle ne présente aucun sens. Les idées qu'elle allie ne sont point *opposées*, mais *disparates*.

33. Il est bon de faire attention à cette distinction entre *opposé* et *disparate*. Si les signes $+$ et $-$ n'avoient qu'une signification, le signe $-$ auroit nécessairement la même signification dans les deux membres de l'équation $\overline{BD}^2=\overline{AB}^2+\overline{AD}^2=\overline{AB}^2-(-\overline{AD}^2)=1^2-1^2=0$. Cette équation offriroit

deux idées *opposées* regardées comme une seule et même idée. Elle seroit contradictoire. *Ce seroit la faute du signe* —. Mais si les signes + et — ont chacun deux significations, l'équation $\overline{AB}^2 + \overline{AD}^2 = 1^2 - 1^2 = 0$ n'offre plus que deux idées *disparates*. Elle est l'effet d'une confusion d'idées. *C'est la faute de l'analiste.*

34. Cette distinction me paroît prévenir les objections insurmontables que Mr. Carnot fait, dans le discours préliminaire de sa *Géométrie de Position*, contre cette proposition : les signes + et — indiquent deux directions opposées. Ses objections supposent tacitement que ces signes n'ont qu'une signification.

35. Autre exemple. Selon la manière ordinaire de s'exprimer, on a (Fig. 1.) $\overline{BD}^2 = 1^2 + 1^2$ Par conséquent $\overline{BD}^2 = (1+\sqrt{-1^2})(1-\sqrt{-1^2})$, et $1+\sqrt{-1^2} : \sqrt{2} :: \sqrt{2} : 1-\sqrt{-1^2}$, proportion absurde, si l'on attribue à $\sqrt{2}$ sa signification arithmétique. Mais si l'on multiplie la seconde raison de cette proportion par $1+\sqrt{-1^2}$, on aura - - - - - - - - - $1+\sqrt{-1^2} : \sqrt{2} :: \sqrt{2} \times \overline{1+\sqrt{-1^2}} : 2$, ou $1+\sqrt{-1^2} : \sqrt{2} \times \overline{1+\sqrt{-1^2}} :: \sqrt{2} : \sqrt{2} \times \sqrt{2}$, proportion dont la vérité saute aux yeux.

J'ai dit : *si on attache à* $\sqrt{2}$ *sa signification arithmétique ;* car si l'on attache à $\sqrt{2}$ sa signification géométrique, qui est de représenter la diagonale d'un carré dont le côté est 1, alors la proportion $1+\sqrt{-1^2} : \sqrt{2} :: \sqrt{2} : 1-\sqrt{-1^2}$ ne sera plus absurde. En effet, si $\sqrt{2}$ représente $\overline{BD}$ (Fig. 1.), il représente une ligne dont la direction, par rapport à $\overline{BA}$, peut être représentée de la manière suivante : soit $\sqrt{-1} = 1 \times e^{\frac{\pi}{2}\sqrt{-1}}$

(e étant la base des logarithmes hyperboliques et π la demi-circonférence d'un cercle dont le rayon est 1); $1 \times e^{\frac{\pi}{2}\sqrt{-1}}$ signifie la ligne $\overline{AD}$ dont la direction est $e^{\frac{\pi}{2}\sqrt{-1}}$. De même $\sqrt{2} \times e^{\frac{\pi}{4}\sqrt{-1}}$ signifiera la ligne $\overline{BD}$ dont la direction est $e^{\frac{\pi}{4}\sqrt{-1}}$, c'est-à-dire, demi-perpendiculaire. Alors la proportion précédente deviendra $1 + 1 \,.\, e^{\frac{\pi}{2}\sqrt{-1}} : \sqrt{2} \,.\, e^{\frac{\pi}{4}\sqrt{-1}} :: \sqrt{2} \,.\, e^{-\frac{\pi}{4}\sqrt{-1}} : 1 - 1 \,.\, e^{\frac{\pi}{2}\sqrt{-1}}$, ou bien $1 + \sqrt{-1} : \sqrt{2}(\cos. 45^\circ + \sin. 45^\circ \sqrt{-1}) :: \sqrt{2}(\cos. 45^\circ - \sin. 45^\circ \sqrt{-1}) : 1 - \sqrt{-1}$; ou bien $1 + \sqrt{-1} : \sqrt{2}\left(\frac{1+\sqrt{-1}}{\sqrt{2}}\right) :: \sqrt{2}\left(\frac{1-\sqrt{-1}}{\sqrt{2}}\right) : 1 - \sqrt{-1}$, qui est identique.

36. Pour repandre sur cette matière autant de clarté qu'il m'est possible, je me proposerai quelques questions à resoudre. La première sera prise de l'ouvrage déjà cité de Mr. Carnot. J'exposerai d'abord la solution qu'il en donne, ensuite la mienne. Ce rapprochement rendra plus sensibles les principes que je m'efforce d'établir dans ce Mémoire.

Problême II.

37. Voici les termes de Mr. Carnot (No. 58.)

" Proposons-nous cette question : une droite $\overline{AB}$ (Fig. 4.)
" étant donnée, trouver sur cette droite un point K, tel que
" le produit des deux segmens $\overline{AK}$, $\overline{BK}$, soit égal à une quan-
" tité donnée ; par exemple, à la moitié du carré de $\overline{AB}$.

" Comme je ne sais encore si le point K doit se trouver sur
" la droite même $\overline{AB}$, ou sur son prolongement, j'établis

" d'abord mon calcul, en supposant que c'est sur la droite " même ; c'est-à-dire, que K tombe entre A et B.

" Cela posé, prenant $\overline{AK}$ pour l'inconnue, je la désigne par " x, et je nomme a, la droite donnée $\overline{AB}$; la condition du " problême me donnera donc $x\ (a - x) = \frac{1}{2}a^2$, ou $x^2 - ax +$ " $+ \frac{1}{2}a^2 = 0$, d'où je tire $x = \frac{1}{2}a \pm \sqrt{-\frac{1}{4}a^2}\left[= \frac{a}{2}(1 \pm \sqrt{-1})\right]$, " c'est-à-dire, que x est imaginaire.

" Je ne conclus pas de là que la solution du problême pro- " posé soit impossible ; mais seulement qu'elle l'est dans la " supposition que j'ai faite, que le point K est placé entre A et " B ; c'est-à-dire, que le problême a pu être mal mis en équa- " tion, parce que j'aurai établi mes raisonnemens sur une " figure qui n'étoit pas celle que je devois considérer, ou qui " ne pouvoit satisfaire aux conditions du problême. J'établis " donc de nouveau mon raisonnement, en partant d'une hy- " pothèse autre que celle que j'avois faite d'abord, c'est-à- " dire, que je supposerai le point cherché, non sur $\overline{AB}$, comme " je l'avois fait, mais sur un de ses prolongemens, par ex- " emple, en K'.

" Alors la condition du problême me donne $x(x-a) = \frac{1}{2}a^2$, " ou $x^2 - ax - \frac{1}{2}a^2 = 0$; d'où je tire $x = \frac{1}{2}a \pm \sqrt{\frac{3}{4}a^2}$, équation " qui ne contenant plus d'imaginaires résout la question pro- " posée.

" Cette solution est double ; l'une $x = \frac{1}{2}a + \sqrt{\frac{3}{4}a^2}$ étant " positive, résoud sans difficulté la question, conformément à " ma nouvelle hypothèse, c'est-à-dire, en supposant que le " point cherché est sur le prolongement de $\overline{AB}$, au de là du " point B ; ou que le point B se trouve entre A et le point " cherché. Mais l'autre solution $x = \frac{1}{2}a - \sqrt{\frac{3}{4}a^2}$ étant né-

" gative, ne peut se rapporter à la même hypothèse, et " d'après ce qui a été dit ci-dessus, il faut, pour en avoir la " signification, changer le signe, et voir à quel systême corré- " latif l'équation ainsi modifiée pourra satisfaire. Or de ce " changement il résulte, que l'équation $x(x-a)=\frac{1}{2}a^2$, qui " exprime la condition du problême, devient $x(x+a)=\frac{1}{2}a^2$. " Voyons à quel nouveau systême corrélatif peut satisfaire " cette nouvelle expression de la condition du problême.

" Or il est facile de voir que c'est en supposant que le point " K tombe sur le prolongement de $\overline{AB}$, non du côté de B, " comme ci-dessus, mais du côté de A en K″. Et en effet, " en partant de cette nouvelle hypothèse, x sera $\overline{AK''}$, et $\overline{BK''}$ " sera $x+a$; d'où il suit que la condition du problême sera " $x(x+a)=\frac{1}{2}a^2$, et cette équation donnera $x=-\frac{1}{2}a\pm\sqrt{\frac{3}{4}a^2}$ " dont la racine positive est effectivement la même que celle " qui s'étoit présentée négativement."

38. Dans cette solution, Mr. CARNOT raisonne rigoureusement d'après les principes fondamentaux de l'algèbre considérée comme *arithmétique universelle.* Ces principes n'admettant que des unités abstraites ne peuvent admettre de quantités négatives par elles-même, et à plus forte raison, de *carrés négatifs.* Conséquemment à ces principes, Mr. CARNOT est obligé de changer sa première équation en une seconde qui en diffère essentiellement, et celle-ci en une troisième qui ne diffère de la seconde qu'en apparence. En effet

sa première équation est $-x^2+ax-\frac{1}{2}a^2=0$ (1);
sa seconde - - - $+x^2-ax-\frac{1}{2}a^2=0$ (2),
et sa troisième - - $+x^2+ax-\frac{1}{2}a^2=0$ (3).

39. Or je dis que la première diffère *essentiellement* de la seconde. Si l'on met dans l'une et dans l'autre y^2 à la place de o, on aura $-x^2+ax-\frac{1}{2}a^2=y^2$ - - - (4), et -

$$+x^2-ax-\frac{1}{2}a^2=y^2 \quad - - - (5).$$

La première de ces équations appartient à un cercle dont le rayon est imaginaire et dont je donnerai bientôt la description. La seconde appartient à une hyperbole équilatère dont l'axe est $\frac{a}{2}\sqrt{3}$.

40. Je dis que la seconde ne diffère de la troisième qu'en apparence. En effet ces deux équations donnent (en y mettant y^2 au lieu de o) - - - $x^2-ax-\frac{1}{2}a^2=y^2$ - - (5);

$$x^2+ax-\frac{1}{2}a^2=y^2 \quad - - (6).$$

Elles appartiennent à la même hyperbole équalitère dont les axes sont $=\frac{a}{2}\sqrt{3}$. La première exprime les deux branches positives et sousentend les négatives. La seconde exprime les deux branches négatives et sousentend les positives.

41. Dans les principes de Mr. Carnot qui sont, je le répéte, les principes fondamentaux de l'algèbre considérée comme *arithmétique universelle*, l'équation (1) ne donne aucune solution; la seconde n'en donne qu'une, et la 3e. une.

Dans les principes que j'ai exposés et qui appartiennent à l'*algèbre-langue*, ces trois équations donnent chacune deux solutions. Les deux solutions données par la seconde sont les mêmes que celles données par la troisième. Ainsi l'on peut supprimer l'une des équations. L'équation restante donnera les deux solutions de Mr. Carnot. Quant à la première, il faut développer les deux solutions qu'elle donne dans mes principes.

42. Soit (Fig. 5.) AK $=+\frac{a}{2}$, BK $=-\frac{a}{2}$, KC (décrit sur le

plan de ce papier) $= + \frac{a}{2}\sqrt{-1}$, KD $= -\frac{a}{2}\sqrt{-1}$, KE (décrit perpendiculairement au plan de ce papier) $= + \frac{a}{2}\sqrt{-1}$, KG $= -\frac{a}{2}\sqrt{-1}$.

CEDG est un cercle décrit du centre K et du rayon $\frac{a}{2}\sqrt{-1}$, perpendiculairement au plan de ce papier.

Le même cercle peut être décrit d'un mouvement conique par l'apothême AE dont une extrémité seroit fixe au point A. Ce cercle CEDG ainsi décrit est celui qui satisfait à l'équation (4).

43. En effet changeons, dans cette équation, x en $\left(z + \frac{a}{2}\right)$, c'est-à-dire, transportons l'origine des x, de A en K. Cette équation deviendra $-z^2 - \frac{a^2}{4} = y^2$, ou $y^2 = \left(\frac{a}{2}\sqrt{-1}\right)^2 - z^2$ (7).

Elle représente le cercle CEDG décrit du centre K et du rayon $\frac{a}{2}\sqrt{-1}$.

Remettons l'origine des abscisses en K. Nous reviendrons à l'équation (4), et les abscisses seront $x = \frac{a}{2} + z$. Or $\frac{a}{2}$ = AK est décrit sur le plan de ce papier. z au contraire étant une partie du rayon KE $= \frac{a}{2}\sqrt{-1}$, ou KG $= -\frac{a}{2}\sqrt{-1}$, est perpendiculaire à AK. Donc l'abscisse x est une ligne brisée formée de deux lignes perpendiculaires l'une à l'autre. Donc le nouveau rayon est la ligne brisée AKE. Si l'on fait tourner ce rayon sur AK, on aura le cercle CEDG. Or faire tourner le rayon AKE sur AK, c'est faire tourner en même temps AE d'un mouvement conique. Donc, comme je l'ai dit, le cercle CEDG décrit d'un mouvement conique par l'apothême AE, dont une extrémité est fixe au point A, est celui qui satisfait à l'équation (4).

44. Dans cette équation (4), y est une ordonnée prise sur le rayon $KC = +\frac{a}{2}\sqrt{-1}$, ou $KD = -\frac{a}{2}\sqrt{-1}$. Si l'on suppose $y = 0$, alors on a - - - - - - -

$$x = AKE = \frac{a}{2} + \frac{a}{2}\sqrt{-1} = \frac{a}{2}(1+\sqrt{-1}), \quad - - -$$

ou $x = AKD = \frac{a}{2} - \frac{a}{2}\sqrt{-1} = \frac{a}{2}(1-\sqrt{-1})$.

Ces deux valeurs de x sont les deux racines de l'équation (1) qui est la première de Mr. Carnot.

45. Pour voir maintenant comment elles résolvent son problême, il suffit de l'énoncer de la manière suivante :

Une droite $\overline{AB}$ (Fig. 5.) *étant donnée, trouver sur cette droite un point* K (projection de la ligne $\overline{KE}$ sur la ligne $\overline{AB}$) *tel que le produit des* (deux lignes $\overline{AE}$, $\overline{BE}$ dont les) *deux segments* $\overline{AK}$, $\overline{BK}$ (sont les projections) *soit égal à la moitié du carré de* $\overline{AB}$.

Il suffit donc, pour faire cadrer l'équation (1) avec la question proposée, d'en regarder les *données* comme des *projections*, et d'en rapporter les *demandes*, non à ces projections, mais aux lignes originales.

On pourroit aussi, sans rien ajouter à l'énoncé de Mr. Carnot, supposer que le point K n'est un point que par rapport au plan de ce papier, c'est-à-dire que, quoiqu'il n'ait ni *longueur* ni *largeur sur* le plan de ce papier, il a une *hauteur au dessus*, et qu'ainsi, à quelque partie de cette hauteur que les lignes $\overline{AE}$ et $\overline{BE}$ puissent se joindre, elles sont censées se joindre à ce qu'on nomme le point K. Alors, dans la description de ce point, on ne feroit abstraction que des deux dimensions qui se trouvent sur le plan de ce papier, sans faire abstraction de celle qui en es dehors (No. 28). Dans ce cas

on pourroit retrancher de l'énoncé précedent toutes les parenthèses que j'y ai mises et qui sont des additions faites à l'énoncé de Mr. CARNOT.

46. Pour montrer maintenant que les lignes $\overline{AE}$, $\overline{BE}$ résolvent la question, c'est-à-dire, que leur produit est égal à la moitié du carré de $\overline{AB}$, il y a une observation à faire.

Ce n'est pas le produit *des lignes* $\overline{AE}$, $\overline{BE}$, mais le produit de leurs *valeurs arithmétiques* qui résoud la question; car *un produit de lignes*, c'est-à-dire, un résultat de *lignes multipliées par des lignes* ne signifie rien. On ne demande pas une *figure géométrique*, mais un *nombre*. Or, pour avoir les valeurs arithmétiques de $\overline{AE}$, $\overline{BE}$, il faut écarter de leurs expressions les signes qui n'ont trait qu'à leurs positions. Sans cette précaution, on confondroit les *signes des valeurs numériques* avec des *signes de position* ou des signes purement descriptifs. Cela posé, il est clair que $\overline{AE} = \overline{BE} = \sqrt{\overline{AK}^2 + \overline{KE}^2} = \sqrt{2\overline{AK}^2} = \overline{AK} \times \sqrt{2}$. Donc $\overline{AE} \times \overline{BE} = 2\overline{AK}^2 = 2\left(\frac{\overline{AB}}{2}\right)^2 = \frac{\overline{AB}^2}{2}$.

Problême III.

47. Quel est le point où se joindront les extrémités D, E, des lignes $\overline{AD}$, $\overline{BE}$ (Fig. 6.) tirées des extrémités A, B, de la ligne $\overline{AB}$, en supposant que la longueur de $\overline{AB}$ soit $2a$, celle de $\overline{AD}$, $\frac{1}{2}a$, et celle de BE, aussi $\frac{1}{2}a$?

Cette question paroît évidemment absurde. Résolvons-la. Soit $\overline{Da} = x$, $\overline{Aa} = y$, $\overline{Eb} = x'$, et $\overline{Bb} = y'$. On a $\overline{AD}^2 = \frac{1}{4}a^2 = x^2 + y^2$, et $\overline{BE}^2 = \frac{1}{4}a^2 = x'^2 + y'^2$.

Or dire que les points D et E doivent *se joindre*, ou dire qu'ils doivent *toucher le même point*, c'est dire la même chose.

Il en est de même des points a et b qui doivent être l'un et l'autre sur le point C, milieu de la ligne $\overline{AB}$. On a donc $\overline{Aa} = y = a$, et $\overline{Bb} = y' = a$. Par conséquent $\overline{AD}^2 = \frac{1}{4}aa = x^2 + a^2$, et $\overline{BE}^2 = \frac{1}{4}aa = x'^2 + aa$. Ces deux équations donnent $x = x' = \pm \frac{\sqrt{-3}}{2} . a$, ou $x = \pm \frac{\sqrt{3}}{2} . a \sqrt{-1}$, quantité imaginaire, comme on devoit bien s'y attendre.

48. Montrons cependant que cette quantité imaginaire, indique un sens raisonnable dont la question proposée est susceptible.

Il est certain que, tant qu'on regardera les lignes $\overline{AD}$, $\overline{BE}$, comme des lignes étendues seulement en longueur, sans aucune largeur, et le point avec lequel elles doivent coincider, comme un point sans extension, la question sera impossible (No. 28).

Mais le signe $\sqrt{-1}$ que renferme la solution indique ce qui peut la rendre possible, en indiquant une *largeur* dans les lignes $\overline{AD}$, $\overline{BE}$, ou une *extension* dans le point C.

49. En effet, ce signe $\sqrt{-1}$ qui est attaché à x dont la valeur est $\sqrt{3} \times \frac{a}{2}$, ou $\sqrt{3} \times \overline{AD}$, montre que x doit être perpendiculaire à $\overline{AD}$ (No. 10).

D'après cela, on peut supposer que les lignes $\overline{AD}$ et $\overline{BE}$, dont la longueur est $\frac{a}{2}$, ont une largeur $= \sqrt{3} \times \frac{a}{2}$. Dans cette supposition, ces lignes deviennent des rectangles $\overline{\overline{ADCF}}$, $\overline{\overline{BDCF}}$,* qui se joignent au point C. Dans cette même

* J'adopte ici, et dans le courant de ce Mémoire, la notation de Mr CARNOT. Dans cette notation les lignes sont désignées par des lignes mises au dessus des lettres qui indiquent leurs extrémités. Les parallélogrammes le sont par de doubles lignes, les angles par des lignes brisées, et les courbes par des lignes courbes.

supposition, quoique les extrémités D et E des lignes $\overline{AD}$ et $\overline{BE}$ ne se joignent pas, elles sont les projections du point de jonction C sur les lignes $\overline{AD}$ et $\overline{BE}$ dont elles sont les extrémités.

50. On peut encore supposer que le point de jonction, au lieu d'être sans extension, en a une, et qu'au lieu d'être un cercle infiniment petit dont le rayon est o, c'est un cercle fini dont le rayon est $\sqrt{3}\,\frac{a}{2}$. Dans cette supposition, les lignes sans largeur $\overline{AD}$, $\overline{BE}$ (Fig. 7.) sont tangentes au même cercle DHEE'KD (regardé comme faisant la fonction d'un point) puisqu'elles sont perpendiculaires aux extrémités de ses rayons $\overline{CD}$, $\overline{CE}$.

51. On peut réunir les deux suppositions précédentes. Par exemple, on peut donner aux lignes $\overline{AD}$, $\overline{BE}$, les largeurs $\overline{Dd}$, $\overline{Ee}$, prises à volonté, et attribuer au point C l'étendue du cercle $\overset{\frown}{dhee'kd'}$. Dans cette nouvelle supposition, les rectangles $\overline{\overline{ADdf}}$, $\overline{\overline{BEeg}}$, sont tangens au cercle $\overset{\frown}{dhee'kd'}$.

52. Enfin, au lieu de rectangles, on pourroit supposer des cylindres, et au lieu de cercles, des sphères.

53. Les descriptions que je viens de donner n'ont rapport qu'à la valeur positive de x ou de x', qui est $+\frac{\sqrt{-3}}{2}\,.\,a$. Pour avoir celles qui ont rapport à sa valeur négative $-\frac{\sqrt{-3}}{2}\,.\,a$, il suffit de transporter au dessous de la ligne $\overline{AB}$ tout ce qui se trouve au dessus, dans la Figure 7, et *vice versâ.*

Problême IV.

54. Soient deux plans carrés. Que le côté de l'un excède le côté de l'autre, de deux piés, et que le nombre des piés carrés contenus dans les deux pris ensemble soit 1. Quelles sont les dimensions de ces deux plans carrés?

Je désigne par x le côté d'un de ces carrés, et par $\overline{x+2}$ le côté de l'autre. On a donc, par la question, l'équation $x^2+(x+2)^2=1$, ou $x^2+2x+2=\frac{1}{2}$, qui donne $x=-1\pm\sqrt{-\frac{1}{2}}$. Ces valeurs de x sont imaginaires. La question qu'elles résolvent est impossible, si on suppose en même temps ces carrés sans épaisseur et sans vides. Mais si on leur suppose quelque épaisseur ou quelque vide, alors la question n'est plus impossible.

55. Voyons ce que nous indiquent les signes $+$, $-$, $\pm\sqrt{-1}$. On peut les interpréter, soit géométriquement, soit arithmétiquement.

56. Signes interprétés géométriquement.

Le côté de l'un de ces carrés est $-1\pm\sqrt{-\frac{1}{2}}$, et le côté de l'autre, $+1\pm\sqrt{-\frac{1}{2}}$. -1 et $\pm\sqrt{-1}$ indiquent deux lignes dont l'une est perpendiculaire à l'autre. Par conséquent si $(-1\pm\sqrt{-\frac{1}{2}})$ indique une seule ligne, l'une des deux quantités qui composent $(-1\pm\sqrt{-\frac{1}{2}})$ indique la *longueur* de cette ligne, et l'autre, l'*épaisseur de son point extrême*. Elles ne peuvent pas exprimer, l'une la *longueur* et l'autre la *largeur*, parcequ'il est de l'essence du carré exprimé algébriquement que sa *longueur* et sa *largeur* aient la même expression. Il en est de même de $+1\pm\sqrt{-\frac{1}{2}}$.

Supposons que $\pm\sqrt{-\frac{1}{2}}$ soit la *longueur* de ces côtés. Alors $+1$ sera l'*épaisseur du point extrême*, dans un des carrés demandés, et -1 l'*épaisseur de ce point*, dans l'autre. Soit (Fig. 8.) $\overline{AB'}=\overline{A'B}=1$. On aura $\overline{AA'}=\overline{AB}=\overline{A'B'}=\overline{BB'}=$ (arithmétiquement) $\sqrt{\frac{1}{2}}$. La *longueur* du côté cherché sera donc $\overline{AA'}$, et l'*épaisseur de son point extrême*, $\overline{AB'}$.

57. Il s'agit de savoir si ces valeurs résolvent la question proposée.

La première condition de cette question est que le côté d'un des carrés excède le côté de l'autre, de deux piés. Pour savoir de combien une ligne surpasse une autre ligne, il faut les poser l'une sur l'autre. Ce qui reste découvert est l'excédent. Lorsque ces lignes sont les côtés de deux carrés, ce sont les carrés qu'il faut poser l'un sur l'autre.

Faisons donc cette opération. Pour cela j'élève le plan du carré $ABA'B'$ dont le côté est $+\sqrt{-\frac{1}{2}}$ ou $-\sqrt{-\frac{1}{2}}$ perpendiculairement au plan de ce papier, de sorte que son côté $\overline{AB}$ se trouve sur ce papier même, comme on le voit en $\overline{AB}$, Figure 9. J'applique ensuite, par la pensée, le plan du second carré sur celui du premier. Ce second plan est aussi $=ABA'B'$, et son côté $=\overline{AB}$. La Figure 9. nous montre les côtés $\overline{AB}$ de nos carrés appliqués l'un sur l'autre. L'*épaisseur du point extrême* de l'un est $\overline{Aa}=\overline{Bb}=+1$, et *celle du point extrême* de l'autre, $\overline{Aa'}=\overline{Bb'}=-1$. Ces épaisseurs restent à découvert. Leur somme est $+1+1=2$; car les signes $+$ et $-$ que portent les valeurs de ces épaisseurs représentent leurs *directions*, puisqu'ils sont interprétés géométriquement. Ils ne signifient pas que l'une doive être ôtée de l'autre.

58. Ce n'est donc pas *la différence des longueurs* des côtés qui est égale à 2, comme le demande la question, mais *la somme des épaisseurs de leurs points extrêmes.* Cette solution peut cependant se rapprocher de ce que demande la question, en la considérant ainsi: l'excédent du côté d'un carré sur l'autre est égal à la différence de position de l'une de leurs extrémités, tandis que l'autre extrémité coïncide, dans les deux carrés. Ainsi, dans la Figure 9, les extrémités B et B coïncident, et les extrémités *a* et *a'* sont distantes l'une de l'autre, d'une longueur $= \overline{aa'}$. La question demande que cette distance soit $= 2$, relativement à la *longueur* des côtés. La solution la donne $= 2$, relativement à l'*épaisseur de leurs points extrêmes*, c'est-à-dire, relativement à une seconde *longueur* perpendiculaire à la première.

59. La seconde condition de nôtre question est que le nombre des piés carrés contenus dans les deux carrés pris ensemble soit 1. Or l'*épaisseur* des côtés ne doit entrer pour rien dans ce nombre de piés carrés, puisque des piés carrés sont des surfaces telles que, pour les mesurer, on ne promène la mesure que dans deux dimensions.

Cela posé, le côté d'un des carrés est $+1 \pm \sqrt{-\frac{1}{2}}$ (en y comprenant la longueur et l'épaisseur; comme on le doit, puisqu'ils ont une épaisseur) et le côté de l'autre est $-1 \pm \sqrt{-\frac{1}{2}}$. Les deux carrés sont donc - - -

$$(-1 \pm \sqrt{-\tfrac{1}{2}})^2 = \tfrac{1}{2} \mp 2\sqrt{-\tfrac{1}{2}} = \tfrac{1}{2} \mp \sqrt{-2}, \quad - - -$$

$$\text{et } (+1 \pm \sqrt{-\tfrac{1}{2}})^2 = \tfrac{1}{2} \pm 2\sqrt{-\tfrac{1}{2}} = \tfrac{1}{2} \pm \sqrt{-2}.$$

Dans ces carrés, $\frac{1}{2}$ et $\frac{1}{2}$ sont *les carrés des longueurs;* $\mp 2\sqrt{-\frac{1}{2}}$ et $\pm 2\sqrt{-\frac{1}{2}}$ sont *les sommes des épaisseurs des côtés de ces carrés.*

Par conséquent $\frac{1}{2}$ ét $\frac{1}{2}$ sont les faces. Donc *la somme des faces* des deux carrés est 1, comme la question le demande.

60. *Remarques.* 1°. On a vû que, dans la mesure des côtés, ce sont *les épaisseurs des points extrêmes*, et dans celle des carrés, les *faces* qu'il faut considérer. Cette circonstance est indiquée par les expressions de ces *côtés* et de ces *carrés*. Les expressions des côtés (qui sont $\overline{+1 \pm \sqrt{-\frac{1}{2}}}$ et $\overline{-1 \pm \sqrt{-\frac{1}{2}}}$) indiquent que les *longueurs* ($\pm \sqrt{-\frac{1}{2}}$) sont des perpendiculaires etrangères à la question, et que *les épaisseurs des points extrêmes* n'en sont pas. Les expressions des *carrés* (qui sont $\overline{\frac{1}{2} \pm 2\sqrt{-\frac{1}{2}}}$ et $\overline{\frac{1}{2} \mp 2\sqrt{-\frac{1}{2}}}$) indiquent que les *épaisseurs des côtés* sont imaginaires, et deviennent par là les perpendiculaires étrangères à la question, et que les *faces* sont réelles, comme elles doivent l'être, puisqu'elles sont l'objet de la question. C'est ce que représentent les Figures 9 et 10. Dans la première, les *épaisseurs des points extrêmes* ($\overline{\mathrm{A}a}$, $\overline{\mathrm{A}a'}$) sont sur le plan de ce papier. Dans la seconde, ce sont les *faces* ($\overline{\overline{\mathrm{ABA'B'}}}$). Or, dans une question où il s'agit de mesurer des lignes et des surfaces qui sont censées décrites sur un même plan, on ne doit mesurer que celles qui sont sur ce plan supposé.

61. 2°. Les côtés des carrés sont les lignes brisées $\widehat{a\mathrm{AB}}$, $\widehat{a'\mathrm{AB}}$ (Fig. 9). Les carrés même sont les plans brisés $\widehat{\widehat{a'\mathrm{A'A}ab\mathrm{BB'}}}$, $\widehat{\widehat{a''\mathrm{A'A}cb'\mathrm{BB'}}}$ (Fig. 10). Les lignes brisées présentent deux dimensions. Les plans brisés en présentent trois.

62. 3°. Dans la Figure 10, les plans qui expriment les épaisseurs des côtés ne sont établis que sur deux des côtés du

carré. Mais on pourroit les établir sur les quatre côtés sans que la solution du problême en souffrît. C'est une suite de l'indétermination dont il a été parlé au No. 15. Dans ce cas, les deux carrés que demande le problême se trouveroient former une boite dont ils seroient les deux moitiés. Cette boite ne contiendroit que des plans sans épaisseur. Les épaisseurs des côtés des carrés demandés seroient les hauteurs des ces deux moitiés de boite. Les carrés demandés seroient ses deux fonds, dont l'un supérieur et l'autre inférieur.

63. Signes $+$, $-$ et $\pm\sqrt{-1}$ interprétés arithmétiquement.

. $\pm\sqrt{-1}$ n'a aucune signification arithmétique (No. 10); mais $\pm\sqrt{-1} \times \pm\sqrt{-1}$ $(= -1^2)$ en a une. -1^2 est un *carré soustractif*, et par conséquent formant *un vide* au milieu d'une surface pleine. De là il suit que, lorsqu'il s'agit de carrés et, en général, de plans, le signe $\pm\sqrt{-1}$ peut indiquer des vides, au lieu de perpendiculaires.

64. Mais les perpendiculaires ne diminuent ni n'augmentent les longueurs des lignes auxquelles elles sont perpendiculaires. Si donc on substitue des *vides* aux perpendiculaires et des *pleins* aux lignes auxquelles elles sont perpendiculaires, ces vides ne doivent pas diminuer ces pleins. Il faut donc faire une *addition de vide*, et non une *soustraction* de plein. Or ajouter des vides à une surface, c'est augmenter ses dimensions sans augmenter le nombre des piés carrés qu'elle contient; c'est l'augmenter *géométriquement*, sans l'augmenter *arithmétiquement*. De même, ajouter $+a\sqrt{-1} - a\sqrt{-1}$ à une quantité qui se *mesure* et qui se *compte*, c'est ajouter à sa *mesure* sans ajouter à sa *somme*. En effet, les signes $+$ et $-$

pris *géométriquement* indiquent des directions opposées, *en partant du même point*, et par conséquent des directions qui s'ajoutent ; au lieu que ces mêmes signes pris *arithmétiquement* se détruisent l'un l'autre.

65. La question proposée contient deux parties, l'une géométrique, l'autre arithmétique. La partie géométrique est *la disposition* des lignes et des surfaces (No. 2). La partie arithmétique est *le calcul* de nombre des piés linéaires contenus dans les côtés qui sont des lignes, et de celui des piés carrés contenus dans les carrés même qui sont des surfaces.

Or, dans cette seconde solution, où les signes $+$, $-$, $\pm\sqrt{-1}$ sont pris arithmétiquement, il ne peut être question que de la partie arithmétique du problême. Je puis donc ajouter tant de vides que je voudrai, puisqu'ils n'altèrent pas la solution arithmétique. Je puis donc résoudre la question proposée en réduisant les deux surfaces carrées à des cadres carrés tels, que le côté de l'un surpasse le côté de l'autre, de deux piés, et que chacun de ces cadres ait une surface égale à un demi pié-carré.

66. Il y a une infinité de manières de résoudre la question proposée, au moyen de cadres de cette espèce, puisqu'on n'a que deux conditions à remplir et quatre variables pour les remplir. Les deux conditions sont, la différence des côtés qui doit être $=2$, et la somme des carrés qui doit être $=1$. Les quatre variables sont, 1°. le côté du premier carré, 2°. celui du second, 3°. le côté du carré vide renfermé par l'un des cadres, 4°. le rapport entre la surface du premier cadre et celle du second. Dans la solution, on a fait ces surfaces égales chacune à la moitié d'un pié carré ; mais rien n'oblige à les faire égales.

67. *Remarques.* 1°. La différence entre les deux solutions précédentes consiste en ce que, dans la seconde, la différence des côtés est exprimée par *la différence de leurs longueurs* qui est une quantité arithmétique ; au lieu que, dans la première, elle est exprimée par *la différence de situation* de ces côtés qui est une quantité géométrique (No. 2).

68. 2°. On pourroit réunir les deux solutions précédentes et résoudre le problême au moyen d'une boite carrée dont les deux fonds contiendroient en somme un pié carré de *plein* et tant de *vides* qu'on voudroit, et dont les hauteurs seroient chacune d'un pié avec *tant et si peu de plein* qu'on jugeroit à propos.

On voit par là, pour le dire en passant, combien la question proposée qui sembloit précise l'étoit peu. Il est certain qu'elle peut être entendue de toutes les manières qu'offrent les solutions précedentes. Si cependant on ne veut pas y reconnoître toutes ces significations, on doit voir du moins que les expressions de la langue algébrique qui correspondent à celles du langage ordinaire sont infiniment plus étendues que celles-ci. Elles ne sont pas vagues, puisqu'on peut en trouver toutes les significations, à l'aide de quelques principes ; mais elles sont générales.

Problême V.

69. Un marbrier a deux cubes de marbre. Le côté d'un de ces cubes excède le côté de l'autre, de deux piés, et le nombre des piés cubes contenus dans les deux est 28. Quelles sont les dimensions de ces deux cubes ?

Avant de donner la solution de cette question, j'ai une remarque à faire.

Cette question conduit à une équation du 3e. dégré. Toute équation du 3e. dégré a au moins une racine réelle. Par conséquent si, au lieu de 28, qui est le nombre des piés cubiques contenus dans les deux cubes, on n'avoit, par exemple, que 3 piés et $\frac{1}{4}$, on devroit encore avoir une solution possible. Cette solution donneroit pour le nombre des piés cubiques contenus dans un des cubes, $\frac{27}{8}$, et dans l'autre, $-\frac{1}{8}$. Or, pour que ce résultat qu'on appelle possible eût un sens raisonnable, il faudroit supposer qu'un des deux cubes fût un vide fait dans l'autre, c'est-à-dire, qu'il faudroit supposer un cube de $\frac{27}{8}$ pouces cubiques contenant un vide de $\frac{1}{8}$ de pouce cubique. Mais cette solution est toute semblable à celle qu'ont fournie les racines imaginaires de l'équation du problême précédent. Les deux solutions ont donc la même espèce de possibilité, quoique l'une soit donnée par un résultat imaginaire et l'autre par un résultat qui ne l'est pas.

La solution même du problême précédent répond mieux à l'énoncé de la question que la solution de celui-ci ; car, dans la premiére de ces questions, on demande deux plans carrés, c'est-à-dire, deux étendues qui aient chacune deux dimensions égales. Or une étendue peut être vide. Dans la seconde question, au contraire, on demande deux cubes *de marbre*. Or un cube de marbre n'est pas un cube de vide.

70. Revenons à nôtre 5e. problême.

Ce problême ne présente à l'esprit qu'une solution possible, et l'équation du 3e. dégré qui en exprime les conditions n'a qu'une racine réelle.

Cette équation est $x^3 + (x+2)^3 = 28$ - - (7).

Ses trois racines sont - - - - - - -

$x = 1$. - - - - - - dont le cube est $x^3 = 1$;
$x = -2 + \sqrt{-6}$ - - dont le cube est $x^3 = 28 + 6\sqrt{-6}$;
$x = -2 - \sqrt{-6}$ - - dont le cube est $x^3 = 28 - 6\sqrt{-6}$.

Ces racines sont les côtés des cubes x^3.

Les côtés des cubes $(x+2)^3$ sont
$x + 2 = 3$ - - dont le cube est $(x+2)^3 = 27$;
$x + 2 = +\sqrt{-6}$ dont le cube est $(x+2)^3 = -6\sqrt{6}$;
$x + 2 = -\sqrt{-6}$ dont le cube est $(x+2)^3 = +6\sqrt{-6}$.

En raisonnant sur les deux dernières des racines x et $\overline{x+2}$ comme on a raisonné sur les deux racines $-1 \pm \sqrt{-\frac{1}{2}}$ qui résolvent la question précédente, on parviendra à des résultats semblables, l'un géométrique et l'autre arithmétique. Il y a cependant ici quelques remarques à faire.

71. 1°. Pour que le résultat géométrique soit juste, il faut que les mesures qu'il fournit remplissent les conditions de la question. Or si l'on raisonne relativement aux cubes comme on l'a fait (No. 60.) relativement aux carrés, on verra qu'il ne faut mesurer que ce qui ne porte pas le signe $\sqrt{-1}$. De plus, les mesures données par les termes réels sont justement ce qu'il faut pour satisfaire aux conditions de la question. Il ne faut donc pas mesurer les autres.

Mais il se présente ici une difficulté qui n'a pas lieu dans le cas du No. 60. C'est qu'un des cubes, savoir, $\overline{+\sqrt{-6}}$ ou $\overline{-\sqrt{-6}}$ est tout entier sous le signe $\sqrt{-1}$. Il ne faut donc en mesurer aucune partie. Il est donc étranger à la question. Il n'en remplit donc pas les conditions. Les conditions demandent deux cubes, et l'on n'en trouve qu'un.

Il faut convenir que, dans ce cas, les deux racines imaginaires

de l'équation ne donnent que des solutions impossibles, si on les considère géométriquement, c'est-à-dire, si l'on s'en tient aux lignes même et aux positions de ces lignes fournies par ces deux racines.

72. Elles peuvent cependant résoudre la question, même géométriquement, elles sont même les seules qui le puissent, si l'on énonce cette question de la manière suivante :

Un marbrier se propose de tailler deux cubes de marbre. Il veut que le côté d'un de ces cubes excède le côté de l'autre, de deux piés ; mais il ne peut y employer que 28 piés cubiques de marbre. Cette quantité n'étant pas suffisante pour la grandeur des cubes qu'il veut avoir, il est obligé d'y joindre de faux marbre pour remplir les vides que l'accroissement des dimensions doit occasionner entre les parties du vrai marbre. Cependant il désire, 1°. que la quantité de ce faux marbre qui remplit les vides laissés par le vrai marbre, soit *la plus petite* possible ; 2°. qu'en même temps l'étendue des deux cubes soit *la plus grande* possible. Quelle doit être la quantité de faux marbre ou de vide ? et quelles doivent être les dimensions des deux cubes ?

73. Puisque la quantité de faux marbre ou de vide doit être un *minimum*, l'addition faite au côté du cube composé de vrai et de faux marbre doit être aussi un *minimum*. De plus, puisque cette addition est étrangère au vrai marbre, elle doit être étrangère à ses dimensions, et par conséquent à leur longueur. Donc la ligne qui exprime une quelconque des dimensions du faux marbre doit être perpendiculaire à celle qui exprime la dimension correspondante du vrai marbre. Donc le côté du cube formé de vrai et de faux marbre doit être une ligne brisée.

Soit cette ligne brisée $(x \pm y\sqrt{-1})$, x étant la partie correspondante au vrai marbre, et $\pm y\sqrt{-1}$ la partie correspondante au faux. Nos cubes seront donc $(x \pm y\sqrt{-1})^3$ et $(x + 2 \pm y\sqrt{-1})^3$.

Puisque $y\sqrt{-1}$ doit être un *minimum*, sa variation doit être $= 0$. Puisque x est constant, sa variation doit aussi être $= 0$. Mais il y a ici une remarque à faire.

Si l'on prenoit les variations ou les différentielles de $(x \pm y\sqrt{-1})^3$ et de $(x + 2 \pm y\sqrt{-1})^3$ à l'ordinaire, on regarderoit par là même x et $\pm y\sqrt{-1}$ comme ayant leurs variations distinctes. On prendroit $(x \pm y\sqrt{-1})$, non comme une *seule* ligne brisée, mais comme *deux* lignes distinctes. Le faux marbre n'auroit d'autre *minimum* que zéro, et la question ne seroit pas résolue. Il faut donc lier $\pm y\sqrt{-1}$ à x. Pour cela, il faut les regarder comme le sinus et le cosinus d'un même arc et que cet arc seul varie.

74. Soit u l'arc dont y est le sinus et x le cosinus. Soit de plus e le nombre dont le logarithme hyperbolique est 1. On aura $(x \pm y\sqrt{-1})^3 = e^{l(x \pm y\sqrt{-1})^3} = e^{3l(x \pm y\sqrt{-1})} = e^{3l(\cos. u \pm \sin. x\sqrt{-1})}$

$$\left.\frac{\cos. u \pm \sin. u\sqrt{-1}}{\sqrt{\cos.^2 u + \sin.^2 u}}\right\} \times \sqrt{\cos.^2 u + \sin.^2 u} = e^{3\left\{l\sqrt{\cos.^2 u + \sin.^2 u} + l\left(\frac{\cos. u \pm \sin. u\sqrt{-1}}{\sqrt{\cos.^2 u + \sin.^2 u}}\right.\right.}$$

remarquant que $l\,\dfrac{\cos. u \pm \sin\ u\sqrt{-1}}{\sqrt{\cos.^2 u + \sin.^2 u}} = l\,\dfrac{\cos. u \pm \sin. u\sqrt{-1}}{\text{rayon}} = \pm u\sqrt{-1}\Big\}$

$= e^{3\left\{l\sqrt{x^2+y^2} \pm u\sqrt{-1}\right\}} = e^{3\left\{l(xx+yy)^{\frac{1}{2}} \pm u\sqrt{-1}\right\}}$, dont la différentielle est $\pm 3du\sqrt{-1} \times e^{3l(xx+yy)^{\frac{1}{2}} \pm 3u\sqrt{-1}}$, ou $\pm 3du\sqrt{-1}$ $(x \pm y\sqrt{-1})^3$.

On a donc $\pm 3du\sqrt{-1}$ $(x \pm y\sqrt{-1})^3$ $\pm 3du'\sqrt{-1}$

$.(x+2\pm y\sqrt{-1})^3=0$, en désignant par u' l'arc dont $\pm y\sqrt{-1}$ est le sinus et $\overline{x+2}$ le cosinus. L'intégrale de cette équation est $(x\pm y\sqrt{-1})^3+(x+2\pm y\sqrt{-1})^3+C=0$. Pour déterminer la constante C, je remarque que, quand $y=0$, on doit avoir l'équation (7). Donc $C=-28$, et l'équation du *minimum* est $(x\pm y\sqrt{-1})^3+(x\pm y\sqrt{-1}+2)^3-28=0$.

Cette équation donne pour $x+y\sqrt{-1}$ les mêmes valeurs que l'équation (7.) pour x. On a donc - - -

$x\pm y\sqrt{-1}=1$, ou $=1+0\times\sqrt{-1}$; - - - (8).

$x\pm y\sqrt{-1}=-2+\sqrt{-6}$; - - - - (9).

$x\pm y\sqrt{-1}=-2-\sqrt{-6}$; - - - - (10).

75. Ces valeurs de $(x\pm y\sqrt{-1})$ sont des *minima* et non des *maxima*.

En effet l'équation (10), par exemple, c'est-à-dire, $x\pm y\sqrt{-1}+2+\sqrt{-6}=0$ est la racine d'une équation du second dégré dont l'autre racine est $x\mp y\sqrt{-1}+2-\sqrt{-6}=0$. Cette équation est donc $(x\pm y\sqrt{-1})(x\mp y\sqrt{-1})=$ $=(2+\sqrt{-6})(+2-\sqrt{-6})=+10$. - - -

Faisons $(x\pm y\sqrt{-1})=z$, et $(x\mp y\sqrt{-1})=+v$. Nous aurons $zv=10$, équation à l'hyperbole entre ses assymptotes. Or l'hyperbole est toute convexe par rapport à ses assymptotes. Si donc une de ses ordonnées ou de ses abscisses est ou un *maximum* ou un *minimum*, ce ne peut être qu'un *minimum*. En effet les diagonales des ligne brisées $(x\pm y\sqrt{-1})$ et $(x\mp y\sqrt{-1})$ sont des *minima* par rapport à cette hyperbole.

76. De plus, il est facile de montrer, d'après une autre considération, que la quantité du faux marbre est un *minimum*.

En effet, le vrai et le faux marbre sont liés de manière que leur ensemble forme une seule unité. Par conséquent les quantités de l'un et de l'autre sont en même temps ou des *minima* ou des *maxima*. Or la quantité du vrai marbre est zéro et par conséquent un *minimum* dans l'un des cubes, et la quantité du faux marbre, dans l'autre cube, est aussi un *minimum;* car on ne peut diminuer cette quantité de faux marbre sans diminuer en même temps les 28 cubes du vrai marbre, ou sans altérer la forme cubique.

77. J'ajoute que, si l'on construit les deux cubes comme je vais le dire, on trouvera que leur étendue est un *maximum*, parcequ'on ne pourroit l'augmenter sans opérer une solution de continuité, ou sans que la quantité du faux marbre ne cessât d'être un *minimum*.

D'ailleurs l'équation $(x \pm y\sqrt{-1})^3 + (x + 2 \pm y\sqrt{-1})^3 - 28 = 0$, qui exprime l'étendue des cubes demandés et qui donne un *minimum* pour les côtés $(x \pm y\sqrt{-1})$ et $(x + 2 \pm y\sqrt{-1})$ de ces cubes, donne au contraire un *maximum* pour leur étendue. En effet elle se réduit à celle-ci : - - - - -

$(x \pm y\sqrt{-1} - 1)\{(x \pm y\sqrt{-1})^2 + 4(x \pm y\sqrt{-1}) + 10\} = 0$, ou

$\{x \pm y\sqrt{-1} + 2 + \sqrt{-6}\} \times \{x \pm y\sqrt{-1} + 2 - \sqrt{-6}\} = 0$, ou

(à cause de l'indépendance des signes + et —, et de ce qu'ils ne marquent ici que des directions qui n'affectent point les quantités) - - - - - - - - -

$\{x \pm y\sqrt{-1} + 2 + \sqrt{-6}\} \times \{x \mp y\sqrt{-1} + 2 - \sqrt{-6}\} = 0.$

Cette dernière équation donne - - - -

1°. $(x \pm y\sqrt{-1})(x \mp y\sqrt{-1}) = 10$, équation à l'hyperbole; (11) c'est celle du No. 75;

2°. $x^2 + y^2 = 10$, équation au cercle - - - (12).

Dans l'équation (11), les coordonnées brisées qui expriment les côtés des cubes, ont (comme je l'ai dit) pour diagonale, un axe d'hyperbole qui est un *minimum.*

Dans l'équation (12), le premier membre qui quoique de deux dimensions seulement, exprime une nouvelle unité qui change l'étendue 28 du vrai marbre en une autre étendue plus considerable, ce premier membre, dis-je, représente le carré du rayon d'un cercle, lequel rayon est un *maximum.*

78. Ainsi les deux conditions de la question sont exactement remplies, quoiqu'elles semblent contradictoires. On voit par là que les imaginaires renfermées dans l'équation du problême, bien loin de confirmer cette contradiction, fournissent les moyens de concilier ces conditions.

79. Il ne s'agit plus que de construire les cubes donnés par les valeurs de $x \pm y\sqrt{-1}$ que présentent les équations (8), (9) et (10). Le premier qui est le seul appellé *réel* est spécialement exclu par l'état de la question. Construisons donc le second et le 3e.

Comme le faux marbre est destiné à remplir les vides laissés par le vrai marbre, il ne s'agit que de voir quels seront les vides contenus dans les cubes $(-2+\sqrt{-6})^3$ et $(-2-\sqrt{-6})^3$. Ces cubes sont - - - - -
$-8+12\sqrt{-6}+36-6\sqrt{-6}$, et - - - -
$-8-12\sqrt{-6}+36-6\sqrt{-6}$.

Or ces cubes renferment deux sortes de vides, savoir, 1°. ceux qu'indique le signe $-$; 2°. ceux qu'indique le signe $\sqrt{-1}$. Les premiers sont des *pleins soustractifs.* Les seconds sont des *vides absolus.*

80. En effet 1°. -8 est un cube qui a ses trois dimensions

et qui par conséquent est *plein*, et comme il porte le signe —, il est *soustractif*. 2°. Au contraire $-6\sqrt{-6}$ n'a que deux dimensions pleines.

Pour le prouver, je remarque que $\pm 6\sqrt{-6} = +\sqrt{6}. -\sqrt{6}. \mp\sqrt{-1}\,\sqrt{6}$. Or le signe $\sqrt{-1}$ attaché à $\sqrt{6}$ marque (No. 10.) que $\sqrt{-1}\,\sqrt{6}$ est perpendiculaire à ce qu'il seroit, s'il ne portoit pas ce signe; et s'il ne portoit pas ce signe, on auroit $+\sqrt{6}. -\sqrt{6}. \mp\sqrt{6} = \pm(\sqrt{6})^3$ qui est un cube dont les trois dimensions sont pleines. Puisque les trois dimensions de $\pm(\sqrt{6})^3$ sont pleines, les trois lignes marquées par $+\sqrt{6}$, $-\sqrt{6}$ et $\pm\sqrt{6}$ sont perpendiculaires entr'elles et la ligne marquée par $\pm\sqrt{6}$ est perpendiculaire au plan des deux lignes marquées, par $+\sqrt{6}$ et $-\sqrt{6}$. Si donc $\pm\sqrt{6}$ devient $\pm\sqrt{-1}\,.\sqrt{6}$, la ligne marquée par $\pm\sqrt{-1}\,.\sqrt{6}$ se trouvera nécessairement dans le plan des deux autres lignes. Le cube se trouvera donc réduit à un plan. Il ne sera donc plein que dans deux de ses dimensions. La troisième dimension sera donc ou vide ou nulle.

81. Maintenant je dis qu'elle sera vide et non pas nulle, ou plutôt, qu'elle sera *vide géométriquement et nulle arithmétiquement.*

Comme cette distinction est un point fondamental dans les principes que j'expose, il faut que je l'explique.

Si, après avoir parcouru une toise dans un sens quelconque, je la parcours une seconde fois en revenant au point de départ, le nombre des toises que je parcourrai sera $=2$, et la quantité dont je m'éloignerai du point de départ sera $=0$. Ces deux résultats donnent les deux significations de $\overline{+1-1}$. Le

premier donne sa signification *géométrique*, et le second, sa signification *arithmétique*. Le premier exprime les opérations, et le second en exprime la conséquence. Le premier représente les mesures prises qui sont les données du calcul *à faire*, et le second représente le calcul *fait* d'après ces données. Pour savoir donc si ce sont les données ou leur conséquence qu'exprime $+\sqrt{6}.-\sqrt{6}.\mp\sqrt{-1}\sqrt{6}$, il faut examiner ce qui est demandé par la question. Or, la question est de construire un cube et de déterminer ce qu'il contient de matière réelle. Mais pour construire un cube, il faut avoir son côté et l'étendre dans les trois dimensions. Pour déterminer ce qu'il contient de matière réelle (lorsqu'on sait d'ailleurs qu'il en peut contenir qui ne le soit pas) il faut savoir combien de matière réelle s'étend dans ses trois dimensions. Le premier point de la question, qui consiste à *mesurer*, est géométrique. Le second, qui consiste à *compter*, est arithmétique. Or la réponse au premier point de la question donne *trois* dimensions, et celle au second point n'en donne que *deux*. La troisième dimension est donné *réelle géométriquement et nulle arithmétiquement*. Cependant elle ne peut pas être réelle et nulle en même temps. Elle renferme donc deux choses dont l'une est réelle et l'autre nulle. C'est donc un vide et non une nullité absolue. La matière est nulle; mais l'espace est réel. La 3e. dimension est donc *vide géométriquement et nulle arithmétiquement*. Donc le signe $\sqrt{-1}$ mis devant l'expression d'un cube ou d'un parallélipipède marque *un vide* dont l'étendue est égale à celle de ce cube ou de ce parallélipipède.

82. Il s'agit maintenant d'expliquer la différence qui se

trouve entre ce que j'ai appellé *plein soustractif* et ce que j'ai nommé *vide absolu.** Le plein soustractif est, comme je l'ai dit, désigné par le signe —, et le vide absolu, par le signe $\sqrt{-1}$. La différence entre le plein soustractif et le vide absolu n'est donc que la différence de ce qu'expriment les signes — et $\sqrt{-1}$. Le signe $\sqrt{-1}$ appliqué à un espace qui s'étend dans les trois dimensions, marque *par lui-même* la destruction, non pas de l'espace qui a été rempli, mais de la matière qui le remplissoit. Le signe — marque cette même destruction, mais non *par lui-même.* Il faut, pour qu'il la marque, qu'il soit accompagné du signe +. Ainsi, par exemple, dans le cube $-8-12\sqrt{-6}+36+6\sqrt{-6}$, pour que -8 représente un vide, il faut écrire ce cube ainsi: $-8-12\sqrt{-6}+8+28+6\sqrt{-6}$. Or je dis que $-8+8$ désigne un vide dont la partie -8 s'étend selon la direction marquée par —, et la partie $+8$ s'étend selon la direction marquée par +. Conséquemment, l'espace $-8+8+28$ considéré *géométriquement*, c'est-à-dire, par rapport à son *étendue abstraite*, renferme les trois espaces désignés par 28, -8 et $+8$, savoir, 28 pieds cubiques dans le sens +, 8 cubiques dans le sens + et 8 pieds cubiques dans le sens —. Il renferme donc 44 pieds cubiques. Mais ce même espace considéré *arithmétiquement*, c'est-à-dire, par rapport à son *étendue matérielle* ne contient que l'espace marqué par la valeur

* (N^a.) Ces considérations sur le plein et le vide peuvent paroître plus que singulières dans une question de géométrie; mais puisque j'introduis dans mon analyse des signes qui ont deux significations indépendantes l'une de l'autre, il faut bien que j'obtienne des résultats de deux espèces, c'est-à-dire, des résultats dont l'un ait un caractère que l'autre n'a pas. *La présence* et l'*absence* de ce caractère sont ce que j'appelle *plein* et *vide*, quel que soit ce caractère.

arithmétique de $-8+8+28$ qui est $=28$. Ainsi, tandis que $+6\sqrt{6}\sqrt{-1}$ représente un vide cubique $=6\sqrt{6}$, -8 conduit à un vide $=8\times 2$.

83. Ce résultat, tout paradoxal qu'il est, est la conséquence d'une propriété remarquable des quantités imaginaires, savoir, d'être des *logarithmes*.

N[a]. Qu'il me soit permis ici d'interrompre mon sujet, pour m'arrêter un peu sur les propriétés logarithmiques du signe $\sqrt{-1}$.

84. [*Propriétés logarithmiques du signe* $\sqrt{-1}$. Je dis qu'en général, si l'on regarde $\sqrt{-1}$, non comme la racine (racine impossible) du *carré arithmétique* -1 (carré pareillement impossible), mais comme le signe de l'*opération géométrique* par laquelle on éléve une perpendiculaire, on aura $(\sqrt{-1})^n = \pm n(\sqrt{-1})$.

Pour le prouver, supposons d'abord n un nombre entier impair. Nous aurons $\sqrt{-1} = e^{\frac{\pi}{2}\sqrt{-1}} - e^{-\frac{\pi}{2}\sqrt{-1}} - e^{\frac{\pi}{2}\sqrt{-1}}$

Donc $(\sqrt{-1})^n = e^{\frac{n\pi}{2}\sqrt{-1}} = e^{\left(\frac{\pi}{2}+\frac{\pi}{2}+\frac{\pi}{2}+\text{\&c.}\right)\sqrt{-1}}$
$= e^{(90^\circ+90^\circ+90^\circ+\text{\&c.})\sqrt{-1}}$.

Mais que peut signifier cet exposant $(90^\circ+90^\circ+\text{\&c.})\sqrt{-1}$? Dans ax^n, l'exposant n marque que x multiplie a autant de fois que n renferme d'unités. Ainsi, dans ax^n, n est un signe d'opérations arithmétiques. Mais aucune opération arithmétique ne peut être désignée par $(90^\circ+90^\circ+\text{\&c.})\sqrt{-1}$. Cet exposant est donc le signe d'opérations *purement géométriques* (No. 2), c'est-à-dire, d'opérations où l'on n'a en vue que les *directions*, sans considérer les *longueurs*.

85. Il n'y a nulle analogie entre une *multiplication* et une *direction*. Comment donc se faire une idée claire d'un exposant qui indique *une somme de directions*, quand l'idée qu'on a coutume d'attacher exclusivement à un exposant est, qu'il indique *une somme de multiplications?*

Un seul moyen se présente. C'est de trouver une idée complexe qui renferme les deux idées de *multiplication* et de *direction*. Or *la mesure de l'étendue* présente cette idée complexe (No. 2).

Prenons pour exemple le carré ABCD (Fig. 11). Pour mesurer ce carré, *je porte la mesure* de A en B, puis de B en D, dans une *direction perpendiculaire* à celle de AB. Voilà l'idée de *direction*. Ensuite *je compte* séparément les parties de AB et les parties de BC, puis je *multiplie* le nombre des premières par celui des secondes. Voilà l'idée de *multiplication*. De ces deux idées ne conservons que la première, puisque la seconde est exclue par le signe $\sqrt{-1}$. Dans ce cas, l'exposant $(90^\circ + 90^\circ + \&c.)\sqrt{-1}$ exprimera un simple arc de cercle *perpendiculaire au rayon*, et le nombre des multiplications qu'il exprimera sera $= 0$. De là il suit que, pour ramener l'exposant $(90^\circ + 90^\circ + \&c.)\sqrt{-1}$ aux exposans ordinaires, c'est-à-dire, aux exposans qui sont des signes de multiplications, il faut le multiplier par 0. On aura donc $e^{0\times(90^\circ+90^\circ+\&c.)\sqrt{-1}} = e^{0\times n\,.\,90^\circ\sqrt{-1}} = 1 + le^{0\times n\,.\,90^\circ\sqrt{-1}} =$ $= 1 + 0\times n\,.\,90^\circ\sqrt{-1}\,.\,le =$ (voyés la note cy dessous)* $1 + 0\times n\,.\,90^\circ\sqrt{-1}$.

* On peut objecter contre cette équation que le 0 que j'ai ajouté au terme $n\,.\,90^\circ\sqrt{-1}$ ne le rend point nul, et que par conséquent la véritable équation n'est pas $e^{0\times n\,.\,90^\circ\sqrt{-1}} = 1 + 0\times n\,.\,90^\circ\sqrt{-1}$, mais - - - - - -

L'expression $o \times n \cdot 90^\circ\sqrt{-1}$ renferme deux parties, savoir, $n \cdot 90^\circ\sqrt{-1}$ et o.

Soit (Fig. 11.) l'arc $A\dot{E}I = n \cdot 90^\circ\sqrt{-1}$. Le signe $\sqrt{-1}$ que renferme $n \cdot 90^\circ\sqrt{-1}$ marque que $\widehat{AEI}$ est perpendiculaire au plan de ce papier. En effet si $n \cdot 90^\circ$ ne portoit pas le signe $\sqrt{-1}$, il seroit déja perpendiculaire au rayon $\overline{CA}$. Or le signe $\sqrt{-1}$ ne peut pas le faire cesser d'être perpendiculaire à ce rayon, puisqu'il ne peut pas le faire cesser d'être un arc de cercle décrit de ce rayon. Donc le signe $\sqrt{-1}$

$$e^{o \times n\, 90^\circ\sqrt{-1}} = 1 + o \times n \cdot 90^\circ\sqrt{-1} + \frac{(o \times n \cdot 90^\circ\sqrt{-1})^2}{1 \cdot 2} + \frac{(o \times n \cdot 90^\circ\sqrt{-1})^3}{1 \cdot 2 \cdot 3} + \&c.$$

Pour prévenir cette objection, je remarque

1°. Que ce qui est représenté par une série ne peut être que le résultat d'une suite d'opérations purement arithmétiques; que par conséquent on n'y doit considérer que la valeur arithmétique. Or $o \times n \cdot 90^\circ\sqrt{-1}$ est *arithmétiquement* - - $= o \times n \cdot 90^\circ\sqrt{-1} + \frac{(o \times n \cdot 90^\circ\sqrt{-1})}{1 \cdot 2} + \&c.$ C'est ce qu'il est facile de montrer. En effet si $n \cdot 90^\circ\sqrt{-1}$ exprime quelque chose, ce ne peut être qu'un arc indéfini $\widehat{AEI}$ (Fig. 11.) qui (comme je le prouverai dans le texte) doit être perpendiculaire au plan de ce papier. Cet arc est purement descriptif. La chose dont il détermine la position est la seule qu'on doive considérer dans la série précédente. Mais quelle est cette chose dont l'arc $\widehat{AEI}$ détermine la position? Cette chose, quelle qu'elle soit, doit être sur le plan de ce papier. Or l'arc $\widehat{AEI}$ étant perpendiculaire au plan de ce papier ne peut déterminer sur ce plan que deux points. La chose qu'il y détermine se réduit donc à deux points dont *la valeur arithmétique* est nulle. Donc tous les termes de la série précédente, à l'exception du premier (qui est 1) sont arithmétiquement nuls.

Je remarque 2°, que les termes qui, dans cette série suivent le terme $o \times n \cdot 90^\circ\sqrt{-1}$ n'ajoutent rien à *sa valeur descriptive*. En effet tous ces termes sont rendus descriptifs par *le signe descriptif* $n \cdot 90^\circ\sqrt{-1}$ qu'ils contiennent. Chacun de ces termes se reduit donc au signe descriptif $n \cdot 90^\circ\sqrt{-1}$ répété un nombre quelconque de fois. Cette répétition modifie à la vérité l'étendue de l'arc $\widehat{AEI}$, mais elle ne lui

joint au signe de l'arc de cercle $n \,.\, 90^\circ$ marque une double perpendicularité, c'est-à-dire, la perpendicularité à un plan. Or ce plan ne peut être que celui de ce papier. A l'égard de o, nous l'avons posé pour exprimer une *nullité de multiplication*. $0 \times m \times 90^\circ\sqrt{-1}$ n'exprime donc qu'*une trace de ligne* mise au bout de la ligne $\overline{CA}$ et qui lui est perpendiculaire. Donc, en représentant $0 \times n \,.\, 90^\circ\sqrt{-1}$ par $T\,(\widehat{AEI})$ qui signifie *trace de* $\widehat{AEI}$, nous aurons (supposé $\overline{CA} = 1$) $1 + 0 \times n \,.\, 90^\circ\sqrt{-1} = \overline{CA} + T\,(\widehat{AEI})$. Donc - -

$$\frac{e^{+T(\widehat{AEI})} - e^{-T(\widehat{AEI})}}{2} = \frac{\left\{\overline{CA}+T(\widehat{AEI})\right\}}{2} - \frac{\left\{\overline{CA}-T(\widehat{AEI})\right\}}{2} = T(\widehat{AEI})$$

86. Ainsi, quoiqu'on ait $\dfrac{e^{+\widehat{AEI}\sqrt{-1}} - e^{-\widehat{AEI}\sqrt{-1}}}{2\sqrt{-1}} = \sin.\,\widehat{AEI}$, on a $\dfrac{e^{+T(\widehat{AEI})} - e^{-T(\widehat{AEI})}}{2} = T(\widehat{AEI})$. Pour que ce point paroisse

fait pas décrire d'autres points sur le plan de ce papier. Elle n'ajoute donc rien à sa valeur descriptive.

Je remarque 3°. que la série précédente est $= \cos.\, 0 \times n \,.\, 90^\circ \pm \sin.\, 0 \times n . 90^\circ\sqrt{-1} = 1$. Or cette même série $= 1 +$ une suite de termes dont $0 \times n \,.\, 90^\circ\sqrt{-1}$ est un des facteurs. Désignons cette suite de termes par T. Puisque $0 \times n \,.\, 90^\circ\sqrt{-1}$ est un des facteurs de T, T est donc (arithmétiquement) $= 0$. Il est donc purement descriptif. Or le rayon 1 exprime la distance du cercle et la circonférence. Donc la série entière $(1 + T)$ traduite en langage descriptif signifie : - - - -

la distance 1 *plus la description de l'arc* $\widehat{AEI}$, ou, en d'autres termes ; - -

la trace de l'arc $\widehat{AEI}$ *décrit à la distance* 1 *d'un point pris comme centre.*

Or $e^{0 \times n \,.\, 90^\circ\sqrt{-1}}$ et $1 + 0 \times n \,.\, 90^\circ\sqrt{-1}$ ont l'un et l'autre cette même signification. Donc l'équation $e^{0 \times n \,.\, 90^\circ\sqrt{-1}} = 1 + 0 \times n \,.\, 90^\circ\sqrt{-1}$ est une équation identique.

clair, il suffit de remarquer, 1°. que ce qui est exprimé par $\widehat{AEI}$ renferme deux idées, savoir celle de *nombre* et celle de *direction;* 2°. que lorsqu'on détermine un *sinus*, c'est sa *valeur numérique* seule qu'on détermine; 3°. que le signe $\sqrt{-1}$ exclut toute idée de nombre aussi bien que le signe o; 4°. qu'il en est de même de ce que je me suis proposé de désigner par $T(\widehat{AEI})$; 5° que $\frac{e^{+\widehat{AEI}\sqrt{-1}} - e^{-\widehat{AEI}\sqrt{-1}}}{2\sqrt{-1}}$, en anéantissant le signe $\sqrt{-1}$ de l'exposant par le signe $\sqrt{-1}$ du dénominateur, et en n'admettant pas le signe o, ne laisse que la valeur *numérique;* 6°. qu'au contraire $\frac{e^{+T(\widehat{AEI})} - e^{-T(\widehat{AEI})}}{2}$, en n'anéantissant pas le signe $\sqrt{-1}$ qui exclut toute idée de nombre, et en admettant le signe o, ne laisse subsister que la valeur *descriptive.*

87. On peut donc poser pour principe général, que $\frac{\left(e^{nx\sqrt{-1}} - e^{-nx\sqrt{-1}}\right)}{2}$ *divisé par* $\sqrt{-1}$ *est* $=$ *sin.* nx, *tandis que* $\frac{e^{+0\times nx\sqrt{-1}} - e^{-0\times nx\sqrt{-1}}}{2}$ *non divisé par* $\sqrt{-1}$ *et* $=$ n . T (x).

Or je n'ai mis le signe o que pour ôter, par un signe connu, toute idée de multiplication; mais le signe $\sqrt{-1}$ seul suffit pour exclure cette idée. On peut donc supprimer le zéro. Donc $\frac{e^{+nx\sqrt{-1}} - e^{-nx\sqrt{-1}}}{2} = nT(x)$. Donc $(\sqrt{-1})^n =$

$$\frac{e^{\frac{n\pi}{2}\sqrt{-1}} - e^{\frac{-n\pi}{2}\sqrt{-1}}}{2}$$ (n étant un nombre entier impair)

$: n \,.\, T\left(\frac{\pi}{2}\right) = n(\sqrt{-1})$.

88. Cette équation, toute singulière qu'elle peut paroître, n'est que l'expression algébrique du principe posé au commencement de ce mémoire, savoir, *que* $\sqrt{-1}$ *est la signe de la perpendicularité, abstraction faite de toute longueur de ligne.* En effet le premier membre $(\sqrt{-1})^n$ marque que *le signe* $\sqrt{-1}$ *de la perpendicularité* est répeté n fois, c'est-à-dire, est attaché n fois à la même ligne 1. Le 2d. membre $n \,.\, \mathrm{T}\left(\frac{\pi}{2}\right)$ marque que l'arc $\frac{\pi}{2}$ qui est *la mesure de la perpendicularité* est tracé n fois.

89. J'ai supposé n un nombre entier impair, afin que le signe $\sqrt{-1}$ ne s'anéantît pas. Si n étoit un nombre entier pair, le signe $\sqrt{-1}$ s'anéantiroit, et l'on pourroit croire qu'alors l'équation $(\sqrt{-1})^n = n \,.\, \mathrm{T}\left(\frac{\pi}{2}\right)$ n'auroit pas lieu, puisque (No. 86) j'ai fondé la vérité de cette équation sur ce que le signe $\sqrt{-1}$ qu'elle renferme exclut toute idée de nombre. Mais cette difficulté n'est qu'apparente. En effet lorsque n est un nombre entier pair, ce n'est pas le signe $\sqrt{-1}$ qui disparoit, mais le sinus de l'arc $n \,.\, \frac{\pi}{2}$ qui devient $= 0$. Alors, l'idée de nombre, bien loin de se trouver rétablie, se trouve doublement exclue, savoir, une fois par le signe $\sqrt{-1}$ qui subsiste toujours, et une seconde fois par l'anéantissement de la valeur numérique du sinus. Je dis que le signe $\sqrt{-1}$ subsiste toujours, parcequ'on ne peut l'anéantir qu'en le divisant par lui-même.

90. L'équation $(\sqrt{-1})^n = n \,.\, \mathrm{T}\left(\frac{\pi}{2}\right)$ est donc aussi vraie, lorsque n est un nombre entier positif. Maintenant je dis qu'elle est également vraie, quel que soit n, entier ou fractionnaire, positif ou négatif. Pour cela, il suffit de substituer

à l'idée de *perpendicularité* l'idée d'une *inclinaison* quelconque. Dans ce cas, les deux membres de l'équation - - -

$(\sqrt{-1})^{\pm \frac{n}{m}} = \pm \frac{n}{m} . \mathrm{T}\left(\frac{\pi}{2}\right)$ exprimeront l'inclinaison $\frac{\pi}{2m}$ répétée n fois dans le sens + ou dans le sens —.

91. De ce qui vient d'être dit je tire deux conséquences.

Premiere conséquence. Soit R un rayon quelconque. $R(\sqrt{-1})^n$ exprime la trace d'un arc quelconque $\frac{n\pi}{2}$ décrit du rayon R. Si $R = \frac{r}{0}$ et $n = p \times 0$, $\frac{r}{0}(\sqrt{-1})^{p \times 0}$ exprime la trace d'une ligne droite quelconque. Par là *on peut écrire algébriquement et calculer à la manière des logarithmes les traces d'un nombre quelconque de lignes avec leurs longueurs et leurs positions.* Je regrette de ne pouvoir m'étendre ici sur cette propriété du signe $\sqrt{-1}$ qui paroît devoir être d'une grande utilité dans la géométrie descriptive.

92. Seconde conséquence. *Pour passer du logarithme* $\pm \frac{n}{m} . \mathrm{T}\left(\frac{\pi}{2}\right)$ *à son exponentielle, il suffit de diviser ce logarithme par* $\sqrt{-1}$. *voyez le No.* 86.

93. Revenons à nos cubes de vrai et de faux marbre.

Ces cubes sont $= -8 \pm 12\sqrt{-6} + 36 - 6\sqrt{6}$. Pour avoir le faux marbre qu'ils contiennent, il faut les exprimer ainsi:

$$8(\sqrt{-1})^2 + 12 . \sqrt{6}(\sqrt{-1})^{\pm 1} + 36(\sqrt{-1})^0 + 6\sqrt{6}(\sqrt{-1})^{-}$$

qui donne (No. 84.) - - - - - - - -

$(2 . 8 \pm 12 . \sqrt{6} + 0 . 36 - 6\sqrt{6})\sqrt{-1}$. Le signe $\sqrt{-1}$ marque ici que les signes + et — ne sont pas des signes d'addition et de soustraction. Par consequent la quantité du faux marbre est $2 . 8 + 12\sqrt{6} + 6\sqrt{6} = 16 + 18\sqrt{6}$.

Pour avoir le vrai marbre que contiennent nos cubes, il faut les exprimer ainsi ; - - - - - - -

$-8+12\sqrt{6}\left[\pm T\left(\frac{\pi}{2}\right)\right]+36+6\sqrt{6}\left[-T\left(\frac{\pi}{2}\right)\right]$. Or (No. 86.) le signe $T\left(\frac{\pi}{2}\right)$ anéantit toute valeur numérique. Donc la quantité du vrai marbre se réduit à $-8+36=28$, comme le problême l'exige.

94. *Regle générale.* Etant donné une expression telle que $(A \pm B\sqrt{-1})^{\frac{n}{m}}$ dont le développement contient des termes affectés du signe $\sqrt{-1}$ et des termes qui n'en sont pas affectés, on connoîtra la somme des termes réellement affectés de ce signe, par la regle suivante : Dévéloppez $(A \pm B\sqrt{-1})^{\frac{n}{m}}$ suivant la formule du binome, en observant, 1°. de donner à $\sqrt{-1}$ les exposans indiqués par cette formule, 2°. que $(\sqrt{-1})^{4i}$ (i étant un nombre entier quelconque positif out négatif) fait le même effet que o ; 3°. Qu'il ne faut faire aucune autre réduction que celle de $4i$ à o. Cela fait, tous les termes qui ne seront pas affectés de $(\sqrt{-1})^{4i}$ resteront. Leur somme sera donc celle des quantités réellement affèctées du signe $\sqrt{-1}$.

On voit que, dans ce cas, -1^{2} se trouve être ce qu'on appelle un carré imaginaire.

95. Sur la solution du 5e. problême (No. 70), je remarque 1°. Que l'équation (7.) qui est du 3e. dégré n'est pas dans le cas irreductible. On peut cependant à l'aide des imaginaires, la résoudre comme celles qui sont dans ce cas. C'est ce qu'il faut montrer.

Cette équation développée donne celle ci : - - -
$x^3+3x^2+6x-10=0$ - - - - - (13).
Je fais $x=y-1$, pour anéantir le second terme, ce qui me donne $y^3+3y-14=0$ - - - - - (14).

En imitant le procédé dont on fait usage dans le cas irréductible, si l'on représente par $a\sqrt{-1}$ l'angle dont le sinus est -14 et le rayon $2\sqrt{-1}$, on aura pour les trois racines de l'équation (14) $y=\sin.\frac{a\sqrt{-1}}{3}$, $y=\sin.\left\{\left(60^\circ-\frac{a}{3}\right)\right\}\sqrt{-1}$, et $y=-\sin.\left\{\left(60^\circ+\frac{a}{3}\right)\sqrt{-1}\right\}$, - - - -

ou bien (en multipliant ces sinus par leur rayon $2\sqrt{-1}$, pour les ramener au rayon des tables que je suppose $=1$)
$y=2\sqrt{-1}\times\sin.\frac{a\sqrt{-1}}{3}$, $y=2\sqrt{-1}\times\sin.\left\{\left(60^\circ-\frac{a}{3}\right)\sqrt{-1}\right\}$, et $y=-2\sqrt{-1}\times\sin.\left\{\left(60^\circ+\frac{a}{3}\right)\sqrt{-1}\right\}$.

On verra dans la question suivante (No. 105.) ce que c'est que ces sinus imaginaires plus grands que leur rayon. En attendant, il faut montrer analytiquement que les trois racines qui viennent d'être posées résolvent l'équation (14).

96. Pour donner à cette recherche toute la généralité possible, supposons que l'équation proposée soit $y^3+py+q=0$ (15), et que cette équation ne soit pas dans le cas irréductible, et même que p soit positif.

Je désigne par $a\sqrt{-1}$ l'angle dont le sinus est $\frac{3q}{p}$ (qui, pour l'équation (14), se réduit à -14) et dont le rayon est $2\sqrt{-\frac{p}{3}}$ (qui, pour l'équation (14), se réduit à $2\sqrt{-1}$); et j'ai pour l'équation (15), comme pour l'équation (14),

$$\left\{\begin{array}{l} y=+2\sqrt{-\frac{p}{3}}\times \sin.\frac{a\sqrt{-1}}{3}; \quad - \\ y=+2\sqrt{-\frac{p}{3}}\times \sin.\left\{\left(60^\circ-\frac{a}{3}\right)\sqrt{-1}\right\}; \quad - \quad - \\ y=-2\sqrt{-\frac{p}{3}}\times \sin.\left\{\left(60^\circ+\frac{a}{3}\right)\sqrt{-1}\right\}. \end{array}\right. \qquad (16).$$

97. Pour montrer que ces trois racines sont les vraies racines de l'équation (15), il faut les développer.

$$1^\circ.\ y=+2\sqrt{-\frac{p}{3}}\times\sin.\frac{a\sqrt{-1}}{3}=\sqrt{-\frac{p}{3}}\left\{\left(\times \sin.\frac{a\sqrt{-1}}{3}+\right.\right.$$

$$\left.\left.+\cos.\frac{a\sqrt{-1}}{3}\right)+\left(\sin.\frac{a\sqrt{-1}}{3}-\cos.\frac{a\sqrt{-1}}{3}\right)\right\}.$$

Or supposons que le plan sur lequel l'arc $\frac{a}{3}$ est décrit soit perpendiculaire à celui de ce papier. Si le sinus de cet arc est parallèle au plan de ce papier (comme on est maître de le supposer,) son cosinus sera perpendiculaire à ce même plan. Alors $\sqrt{-\frac{p}{3}}\times\sin.\frac{a\sqrt{-1}}{3}$ restera comme il est, parceque les deux signes de perpendicularité que ce terme renferme se détruisent l'un l'autre. $\sqrt{-\frac{p}{3}}\times\cos.\frac{a\sqrt{-1}}{3}$, au contraire, se changera en $\sqrt{-\frac{p}{3}}\times\cos.\frac{a}{3}$. Donc - - -

$$y=+\sqrt{-\frac{p}{3}}\left\{\left(\sin.\frac{a\sqrt{-1}}{3}+\cos.\frac{a\sqrt{-1}}{3}\right)+\left(\sin.\frac{a\sqrt{-1}}{3}-\cos.\frac{a\sqrt{-1}}{3}\right)\right\}\text{ devient}$$

$$y=+\sqrt{-\frac{p}{3}}\left\{\left(\sin.\frac{a\sqrt{-1}}{3}+\cos.\frac{a}{3}\right)+\left(\sin.\frac{a\sqrt{-1}}{3}-\cos.\frac{a}{3}=\right.\right. \quad - \quad - \quad -$$

$$=+\sqrt{-\frac{p}{3}}\left\{(\sin.a\sqrt{-1}\cos.a)^{\frac{1}{3}}+\left(\sin.\frac{a\sqrt{-1}}{3}-\cos.\frac{a}{3}\right)^{\frac{1}{3}}\right\}= \quad - \quad -$$

$$=+\sqrt{-\frac{p}{3}}\left\{\left(\sin.(a\sqrt{-1})+\sqrt{\text{rayon}^2-(\sin.(a\sqrt{-1}))^2}\right)^{\frac{1}{3}}+\left(\sin.(a\sqrt{-1}\right.\right.$$

$$\left.\left.-\sqrt{\text{rayon}^2-(\sin.(a\sqrt{-1}))^2}\right)^{\frac{1}{3}}\right\}=$$

$$=+\sqrt{-\frac{p}{3}}\left\{\left(\sin.a\sqrt{-1}+\sqrt{1+\sin.^2a}\right)^{\frac{1}{3}}+\left(\sin.a\sqrt{-1}-\sqrt{1+\sin.^2a}\right)^{\frac{1}{3}}\right\}=$$

$$=\left[\sin.(a\sqrt{-1})\times\sqrt{\left(-\frac{p}{3}\right)^3}+\sqrt{\left(-\frac{p}{3}\right)^3+\sin.^2 a\left(-\frac{p}{3}\right)^3}\right]^{\frac{1}{3}}$$

$$+\left[\sin.(a\sqrt{-1})\times\sqrt{\left(-\frac{p}{3}\right)^3}-\sqrt{\left(-\frac{p}{3}\right)^3+\sin.^2 a\left(-\frac{p}{3}\right)^3}\right]^{\frac{1}{3}}.$$

98. Puisque $a\sqrt{-1}$ est l'angle dont le sinus est $\frac{3q}{p}$ et le rayon $2\sqrt{-\frac{p}{3}}$, on a pour le rayon 1, - - -

$$+\sin. a\sqrt{-1}=\frac{+\frac{3q}{p}}{2\sqrt{-\frac{p}{3}}}=\frac{-\frac{q}{2}}{\left(-\frac{p}{3}\right)^{\frac{3}{2}}}.$$ De plus $\sin.(a\sqrt{-1})=(\sin. a)\sqrt{-1}$. Par conséquent $\sin.^2(a\sqrt{-1})=-\sin.^2 a$.

Ainsi $\sin.^2 a=-\sin.^2(a\sqrt{-1})=-\frac{\left(-\frac{q}{2}\right)^2}{\left(-\frac{p}{3}\right)^3}=\frac{\left(\frac{q}{2}\right)^2}{\left(\frac{p}{3}\right)^3}$. La valeur précedente de y devient donc

$$y=\left[-\frac{q}{2}+\sqrt{-\left(\frac{p}{3}\right)^3-\left(\frac{q}{2}\right)^2}\right]^{\frac{1}{3}}+\left[-\frac{q}{2}-\sqrt{-\left(\frac{p}{3}\right)^3-\left(\frac{q}{2}\right)^2}\right]^{\frac{1}{3}}.$$

99. Mais cette valeur de y tire son origine d'un arc perpendiculaire au plan de ce papier, aussi bien que son cosinus qui est $\sqrt{-\left(\frac{p}{3}\right)^3-\left(\frac{q}{2}\right)^2}$ ou $\sqrt{\left(\frac{p}{3}\right)^3+\left(\frac{q}{2}\right)^2}\times\sqrt{-1}$. Si l'on remet cet arc sur le plan de ce papier, son cosinus s'y trouvera et son sinus y restera. Le sinus conservera donc son signe, mais le cosinus perdra le sien qui est $\sqrt{-1}$. On aura donc en dernière analyse - - - - - - -

$$y=\left[-\frac{q}{2}+\sqrt{\left(\frac{p}{3}\right)^3+\left(\frac{q}{2}\right)^2}\right]^{\frac{1}{3}}+\left[-\frac{q}{2}-\sqrt{\left(\frac{p}{3}\right)^3+\left(\frac{q}{2}\right)^2}\right]^{\frac{1}{3}} (17).$$

C'est la formule connue sous le nom de formule de CARDAN.

100. 2°. $y=2\sqrt{-\frac{p}{3}}\times\sin.\left\{\left(60^\circ-\frac{a}{3}\right)\sqrt{-1}\right\}=$ -

$$=\sqrt{-\frac{p}{3}}\Big[\sin.\Big\{\Big(60^\circ-\frac{a}{3}\Big)\sqrt{-1}\Big\}+\cos.\Big(60^\circ-\frac{a}{3}\Big)+$$
$$+\sin.\Big\{\Big(60^\circ-\frac{a}{3}\Big)\sqrt{-1}\Big\}-\cos.\Big(60^\circ-\frac{a}{3}\Big)\Big]=$$
$$=\sqrt{-\frac{p}{3}}\Big[\Big\{\sin.\Big(60^\circ-\frac{a}{3}\Big)\Big\}\sqrt{-1}+\cos.\Big(60^\circ-\frac{a}{3}\Big)+$$
$$+\Big\{\sin.\Big(60^\circ-\frac{a}{3}\Big)\Big\}\sqrt{-1}-\cos.\Big(60^\circ-\frac{a}{3}\Big)\Big]=$$
$$=\sqrt{-\frac{p}{3}}\Big[(\cos.60^\circ+\sin.60^\circ\sqrt{-1})\Big(\cos.\frac{a}{3}-\sin.\frac{a}{3}\sqrt{-1}\Big)$$
$$-(\cos.60^\circ-\sin.60^\circ\sqrt{-1})\Big(\cos.\frac{a}{3}+\sin.\frac{a}{3}\sqrt{-1}\Big)\Big]=$$
$$=\sqrt{-\frac{p}{3}}\Big[(\cos.60^\circ+\sin.60^\circ\sqrt{-1})(\cos.a-\sin.a\sqrt{-1})^{\frac{1}{3}}-$$
$$-(\cos.60^\circ-\sin.60^\circ\sqrt{-1})(\cos.a+\sin.a\sqrt{-1})^{\frac{1}{3}}\Big]=$$
$$=(\cos.60^\circ+\sin.60^\circ\sqrt{-1})\times-\sqrt{-\frac{p}{3}}(\sin.a\sqrt{-1}-\cos.a)^{\frac{1}{3}}$$
$$+(\cos.60^\circ-\sin.60^\circ\sqrt{-1})\times-\sqrt{-\frac{p}{3}}(\sin.a\sqrt{-1}+\cos.a)^{\frac{1}{3}}.$$

Or $\cos.60^\circ=\frac{1}{2}$; $\sin.60^\circ=\frac{\sqrt{3}}{2}$; - - -

$$-\sqrt{-\frac{p}{3}}(\sin.a\sqrt{-1}\mp\cos.a)^{\frac{1}{3}}=(\text{Nos. } 97, 98, 99)\Big[+\frac{q}{2}\pm$$
$$\pm\sqrt{\Big(\frac{p}{3}\Big)^3+\Big(\frac{q}{2}\Big)^2}\Big]^{\frac{1}{3}}.\quad \text{Donc } y=\Big(\frac{1+\sqrt{-3}}{2}\Big)\Big(\frac{q}{2}+ \quad - \quad -$$
$$+\sqrt{\Big(\frac{p}{3}\Big)^3+\Big(\frac{q}{2}\Big)^2}\Big)^{\frac{1}{3}}+\Big(\frac{1-\sqrt{-3}}{2}\Big)\Big(\frac{q}{2}-\sqrt{\Big(\frac{p}{3}\Big)^3+\Big(\frac{q}{2}\Big)^2}\Big]^{\frac{1}{3}}\ (18).$$

C'est la formule générale pour la seconde racine de l'équation (15).

101. 3°. Il est facile maintenant de voir de la 3e. racine de cette équation, laquelle racine est - - -

$$y=2\sqrt{-\frac{p}{3}}\times\sin.\Big\{\Big(60^\circ+\frac{a}{3}\Big)\sqrt{-1}\Big\},\ \text{devient} \quad - \quad -$$
$$y=\Big(\frac{1-\sqrt{-3}}{2}\Big)\Big(\frac{q}{2}+\sqrt{\Big(\frac{p}{3}\Big)^3+\Big(\frac{q}{2}\Big)^2}\Big)^{\frac{1}{3}}+\Big(\frac{1+\sqrt{-3}}{2}\Big)\Big(\frac{q}{2}- \quad -$$

$-\sqrt{\left(\frac{p}{3}\right)^3+\left(\frac{q}{2}\right)^2}\Big)^{\frac{1}{3}}$ (19). C'est la formule générale pour la 3e. racine de l'équation (15).

102. On voit donc par ce détail que les formules (17), (18) et (19) qui sont les formules générales pour la résolution des équations du 3e. dégré, sont les mêmes que les formules (16). Mais celles-ci ne diffèrent des formules pour la résolution de ces mêmes équations, dans le cas irréductible, qu'en ce que l'arc et le rayon portent le signe $\sqrt{-1}$, c'est-à-dire, qu'ils sont dans un plan perpendiculaire à celui où ils seroient dans ce cas. J'ai donc prouvé ce que j'avois intention de prouver, savoir, qu'on peut résoudre les équations du 3e. dégré, dans tous les cas, de la même manière qu'on le fait dans le cas irréductible, c'est-à-dire, *par un sinus*.

103. Il ne me reste plus qu'un point à discuter sur cette matière; c'est de savoir ce que signifient des sinus ou des cosinus imaginaires plus grands que leurs rayons. C'est ce que je vais faire dans la question qui suit.

Problême VI.

104. Que deviennent les courbes du 2d. dégré, c'est-à-dire, les sections coniques, lorsque leurs ordonnées deviennent imaginaires?

Commençons par le cercle. Son équation est, en mettant au centre l'origine des coordonnées, $yy=aa-xx$. Lorsque $x>a$, on a $yy=-(xx-aa)$, et $y=\pm\sqrt{xx-aa}\,.\sqrt{-1}$, ou bien $y\sqrt{-1}=\mp\sqrt{xx-aa}$. Si l'équation $y=\pm\sqrt{aa-xx}$ exprime l'ordonnée d'un cercle décrit sur le plan de ce papier, avec un rayon $=a$, l'équation $y\sqrt{-1}=\mp\sqrt{xx-aa}$

exprime l'ordonnée d'une hperbole équilatère décrite sur un plan perpendiculaire à celui de ce papier, et ayant a pour la valeur de chacun de ses deux demi-axes.

105. Soit ADBE (Fig. 12) le cercle dont l'ordonnée est $y = \pm\sqrt{aa - xx}$, et $AC = a$, le rayon. AmnBpq est l'hyperbole équilatère dont l'ordonnée est $y = \pm\sqrt{xx - aa} \cdot \sqrt{-1}$, ou $y\sqrt{-1} = \mp\sqrt{xx - aa}$. Cette hyperbole est supposée décrite sur un plan perpendiculaire à celui du cercle. $\overline{AC}$, $\overline{CD}$, sont égaux à ses deux demi-axes. $\overline{a'Cd'}$ et $\overline{b'Cc'}$ sont ses assymptotes. Ces assymptotes font avec l'axe $\overline{AB}$ le même angle que les lignes $\overline{aCd}$, $\overline{bCc}$, font avec ce même axe, c'est-à-dire, un angle de 45°.

Or x et y coordonnées du cercle sont le sinus et le cosinus de l'arc qui leur correspond. Donc x et y coordonnées de l'hyperbole équilatère décrite sur un plan perpendiculaire à celui du cercle sont ce que deviennent le sinus et le cosinus, lorsque le sinus devient lui-même plus grand que le rayon.

106. Hyperbole équilatère.

Faisons tourner, par la pensée, la Figure 12 de manière que l'hyperbole AmnBpq se trouve sur le plan de ce papier. Ce mouvement rendra le cercle ADBE perpendiculaire à ce même plan. Par là, l'ordonnée $y\sqrt{-1} = \mp\sqrt{xx - aa}$ de cette hyperbole cessant d'y être perpendiculaire, perd son signe $\sqrt{-1}$, et l'on a $y = \mp\sqrt{xx - aa}$, ou $yy = xx - aa$.

* Lorsque $x < a$, on a $yy = -(aa - xx)$, qui donne $y\sqrt{-1} = \pm\sqrt{aa - xx}$, équation à un cercle perpendiculaire au plan de ce papier.

L'équation $yy = -aa + xx$ donne $(a\sqrt{-1})^2 - (x\sqrt{-1})^2$,

qui diffère de l'équation (7) $y^2 = \left(\frac{a}{2}\sqrt{-1}\right)^2 - z^2$ (No. 43, Probl. II.) en ce que, dans cette dernière équation, l'abscisse z est réelle, au lieu que, dans l'équation $y^2 = (a\sqrt{-1})^2 - (x\sqrt{-1})^2$, l'abscisse $x\sqrt{-1}$ est imaginaire. Cela vient de ce que l'équation du No. 43 est celle d'un cercle décrit d'un mouvement conique, par une ligne brisée, au lieu que l'équation du présent numéro est celle d'un cercle décrit sur un plan perpendiculaire à celui de ce papier.

Ce numéro-ci et les deux précédens montrent que le cercle devient une hyperbole équilatère perpendiculaire, et l'hyperbole équilatère, un cercle perpendiculaire, lorsque leurs ordonnées deviennent imaginaires.

107. Ellipse et hyperbole non-équilatère.

Supposons que toute la Figure 12 tourne également autour de l'axe $\overline{AB}$, sans que ce papier tourne en même temps. Alors, au lieu d'un cercle et d'une hyperbole équilatère, on aura, d'abord par la projection du cercle sur ce papier, une ellipse, ensuite par la projection de l'hyperbole équilatère sur un plan perpendiculaire à ce papier, une hyperbole dont le grand axe sera $= \overline{AB}$, et dont le petit axe sera $< \overline{DE}$.

Les diamètres $\overline{ad}$, $\overline{bc}$, deviendront des diamètres conjugués égaux l'un à l'autre. Leur angle restera égal à l'angle assymptotique, quoiqu'alors $< 90°$.

108. L'équation de cette ellipse rapportée à ses *diamètres conjugués-égaux* sera la même que celle du cercle, savoir, $yy = aa - xx$; mais ses coordonnées feront entr'elles un angle égal à celui des diamètres conjugués.

Il en sera de même des coordonnées de l'hyperbole rapportée à ses assymptotes.

On peut donc raisonner sur cette ellipse et cette hyperbole, comme nous l'avons fait sur le cercle et l'hyperbole équilatère. On y trouvera les mêmes analogies.

109. Parabole.

Son équation est $yy = px$. Si x est négatif, on a $-yy = px$, qui donne $y\sqrt{-1} = \pm px$. Cette seconde équation représente une seconde parabole qui est perpendiculaire au plan de ce papier et qui s'étend dans la partie négative de l'axe de la première parabole supposée décrite sur le plan de ce papier. Les sommets des deux paraboles se touchent, et les directions de leurs axes sont opposées.

110. Prenons maintenant une vue générale de toutes ces sections coniques à ordonnées imaginaires, que j'appellerai *appendices* des sections coniques.

1°. L'hyperbole équilatère est l'appendice du cercle. L'hyperbole non-équilatère est celui de l'ellipse, et la parabole celui de la parabole. Or comme les courbes-appendices sont perpendiculaires à leurs courbes originales, celles-ci le sont à celles-là. Donc le cercle est l'appendice de l'hyperbole équilatère, &c.

111. 2°. L'axe commun à la courbe originale et à la courbe-appendice est la projection de chacune de ces deux courbes sur le plan de son appendice.

112. 3°. Comme toutes ces courbes ont deux axes, savoir, celui des abscisses et celui des ordonnées, il faut dire relativement au second axe tout ce qui vient d'être dit relativement au premier.

113. 4°. Comme les courbes appendices ont les mêmes axes que leurs courbes originales, ces axes ont dans toute leur étendue, c'est-à-dire depuis $-\infty$ jusqu'a $+\infty$, des

ordonnées qui leur correspondent. Or il est facile de voir que ce que j'ai dit des sections coniques peut se dire également de toute autre courbe, soit algébrique, soit transcendante. Donc, supposant l'axe décrit sur le plan de ce papier et appellant *ordonnées* deux espèces de perpendiculaires à cet axe, savoir, celles qui sont sur ce papier et celles qui y sont perpendiculaires, je suis autorisé à établir cette proposition générale: *Une courbe quelconque a dans toute l'étendue de* chacun de ses axes, *c'est-à-dire, depuis* $-\infty$ *jusqu'à* $+\infty$, *autant d'ordonnées que le dégré de son équation contient d'unités, et comme le dégré d'une courbe transcendante est infini, le nombre de ces ordonnées dans tout le cours de chaque axe est infini.*

P. S. Depuis la composition de ce Mémoire, j'ai lû, dans le 1er. Tome des Mémoires de la Societé de Turin, un Mémoire de Mr. De Foncenex intitulé: *Réflexions sur les Quantités imaginaires*, où se trouve l'article suivant:

" No. 6. Si l'on réfléchit sur la nature des racines imaginaires qui, comme on sait, impliquent contradiction entre les données, on concevra évidemment qu'elles ne doivent point avoir de construction géométrique possible, puisqu'il n'est point de manière de les considérer qui lève la contradiction qui se trouve entre les données immuables par elles-même.

" Cependant, pour conserver une certaine analogie avec les quantités négatives, un auteur dont nous avons un cours d'algèbre d'ailleurs fort estimable a prétendu les devoir prendre sur une ligne perpendiculaire à celle où l'on les avoit supposées. Si, par exemple, on devoit couper la ligne

" CB (Fig. 1) $= 2a$ de façon que le rectangle des parties
" $x \times (2a - x)$ fût égal à la quantité $2a^2$, on trouveroit
" $x = a \pm \sqrt{-a^2}$. Pour trouver donc cette valeur de x,
" qu'on prenne sur la ligne CB, la partie AB $= a$, partie
" réelle de la valeur de x, et sur la perpendiculaire DD′, les
" parties AD, AD′, aussi $= a$, on aura les points D, D′, qui
" resolvent le problême en ce que BD $\times$ DC, ou BD′ $\times$ DC′ $= 2a^2$;
" mais puisque les points D, D′, sont pris hors de la ligne CB,
" et qu'une infinité d'autres points pris de même, auroient
" aussi une proprieté semblable, il est visible que, si cette
" construction ne nous induit pas en erreur, elle ne nous fait
" absolument rien connoître. C'est cependant là un des cas
" où elle pourroit paroître plus spécieuse ; car le plus souvent
" on ne voit absolument pas comment le point trouvé pourroit
" résoudre la question, quelque changemens qu'on se permît
" dans l'énoncé du problême.

" Les racines imaginaires n'admettent donc pas une con-
" struction géométrique, et on ne peut en tirer aucun avan-
" tage dans la résolution des problêmes. On devroit par
" conséquent s'attacher à les écarter autant qu'il est possible
" des équations finales, puisque prises dans quelque sens que
" ce soit, elles ne peuvent pas résoudre la question, comme
" les racines négatives dont toute la contradiction consiste
" dans leur manière d'être à l'égard des positives."

Voici les reflexions que cet article m'a suggérées.

1°. La question exposée par Mr. de Foncenex est à peu près la même que celle proposée par Mr. Carnot et qui est le 2d problême du présent écrit. Je ne connoissois pas les objections de Mr. de Foncenex ; mais quand je les aurois

connues, j'aurois dit exactement tout ce que j'ai dit, et rien de plus.*

2°. Ecarter les quantités imaginaires des équations finales, comme Mr. DE FONCENEX le désire, est une chose impossible, passé le second dégré, à moins qu'on n'employe les sinus ou les cosinus, et je prouverai ailleurs que l'expression *finie* des sinus et des cosinus renferme, non seulement de fait, mais *nécessairement*, le signe $\sqrt{-1}$.†

On a vu aux Nos. 95 et suiv. comment la supposition d'un rayon portant le signe $\sqrt{-1}$ m'a conduit aux formules connues pour les équations du 3e. dégré, dont la première ne renferme plus ce signe. Si ce rayon ne portoit pas le signe $\sqrt{-1}$, alors ce signe reparoîtroit *nécessairement* dans cette première formule qui alors représenteroit le cas appellé *irréductible*.

3°. Dans les problêmes que je me suis proposé, je crois avoir fait voir que les racines imaginaires en donnoient des solutions qui n'étoient pas absurdes. On a même vû un problême du 3e. dégré, et par conséquent possible, qui ne pouvoit être résolu par la racine réelle, et qui l'étoit sans absurdité par les deux racines imaginaires.

4°. On a vu un problême (c'est le 3e.) qui paroissoit ab-

* Je n'ai pas cru devoir parler d'un Mémoire de Mr. KHUN qui se trouve dans le 3e. tome des nouveaux Mémoires de Petersbourg, où ce Professeur entreprend de construire les quantités imaginaires, parcequ'il y suppose tacitemet $\sqrt{-1} = -\sqrt{1}$.

† Mr. WOODHOUSE, dans un exellent Mémoire sur les quantités imaginaires inséré dans les *Transactions Philosophiques* de 1801, veut qu'on substitue aux signes *géométriques sin. cos.* &c. les expressions finies imaginaires dont je parle ici, et il regarde ces expressions qui renferment le signe $\sqrt{-1}$ comme représentatives de séries infinies *arithmétiques*. Tout cela est parfaitement conséquent au principe, que *l'algébre n'est qu'une arithmétique universelle.*

surde et dont la solution (par des racines imaginaires) avoit un sens qui ne l'étoit pas. Ce problême n'étoit absurde qu'en apparence. C'étoit une énigme. Les racines imaginaires interprétées selon les principes de ce mémoire en ont donné le mot. Résoudre ainsi un problême, ce n'est pas en changer l'énoncé, c'est l'expliquer; ce n'est pas seulement répondre à une question proposée, c'est encore dire comment elle devoit l'être. Enfin, quelque sens qu'on ait en vue, en proposant une question qui mène à des racines imaginaires, ces racines y satisfont. Si c'est un sens raisonnable, elles le donnent et l'expliquent. Si c'est un sens absurde (ce qui n'arrive que quand on prend ces racines arithmétiquement) elles en montrent l' absurdité, mais elles ne l'expliquent pas; car ce qui est absurde est inexplicable.

5°. J'ai dit que, pour résoudre algébriquement une question, trois choses étoient nécessaires; 1°. traduire la question en langage algébrique; 2°. traduire l'énoncé algébrique de la question en sa solution algébrique; 3°. traduire cette solution algébrique en une solution éxécutive.

Rien ne fait plus d'honneur à l'esprit humain que la sagacité et l'addresse qu'on a mises dans la seconde espèce de ces traductions.

La méthode des variations de Mr. La Grange et l'usage qu'il en fait dans sa *mécanique analytique* offrent des modèles admirables de la première espèce; mais il y manque un point essentiel qu'il étoit impossible à l'algèbre-arithmétique d'obtenir. Je m'explique. Les questions de mécanique roulent sur des quantités concrètes. La manière même dont ces quantités sont concrètes fait partie de ces questions. Ainsi traduire ces quantités concrètes en quantités abstraites, c'est

les traduire imparfaitement. Delà l'impossibilité presque continuelle de parvenir, à l'aide de ces traductions, aux solutions demandées.* Dans les cas même où l'on a réussi, ce n'a été qu'avec une difficulté extrême. Quelle profondeur, quelle addresse, quelle sagacité n'a t'il pas fallu à Mr. LA PLACE pour poser, avec le seul secours des équations différentielles connues (expliquées à la manière ordinaire) la dernière pierre à l'édifice NEWTONIEN ! Avec les seuls signes $+$ et $-$, on ne peut traiter que la géométrie *mensurative ;* cependant les problêmes de mécanique n'appartiennent pas plus à cette géométrie qu'à la géométrie *descriptive.*

A l'égard de la 3e. espèce de traduction qui n'est pas la moins importante, on s'en est beaucoup moins occupé que des deux autres. Aussi je puis citer un problême célébre en mécanique (celui des *cordes vibrantes*) dont la solution, quoiqu'elle fasse le plus grand honneur aux géomètres célébres qui l'ont donnée, n'est certainement pas complette. En effet, ils ont résolu l'équation aux différences partielles $\left(\frac{ddy}{dx^2}\right)=\left(\frac{ddy}{dt^2}\right)$, et leur solution donne la courbe indéterminée qui peut être formée par la longueur de la corde ; mais ils ne résolvent pas l'équation $-\left(\frac{ddy}{dx^2}\right)=-\left(\frac{ddy}{dt^2}\right)$ qui est la véritable équation du problême et qui donneroit la courbe décrite par un point quelconque de cette corde, dans un plan perpendiculaire à son axe. L'équation $-\left(\frac{ddy}{dx^2}\right)=-\left(\frac{ddy}{dt^2}\right)$ est la même (arithmé-

* On a regardé comme un trait de génie dans Mr. LA GRANGE d'être parvenu à traiter de la mécanique la plus sublime, sans figures. C'en a été un plus grand d'avoir vu qu'il n'en falloit pas. En effet, traiter la mécanique algébriquement, c'étoit (dans le systême d'algèbre généralement adopté) ne la traiter qu'arithmétiquement. Or l'arithmétique n'a pas besoin des figures de la géométrie.

tiquement que l'équation $+\left(\frac{ddy}{dx^2}\right) = +\left(\frac{ddy}{dt^2}\right)$; mais elle n'est pas la même relativement à la géométrie descriptive. L'intégrale de la première est très différente de l'intégrale de la seconde. Elle est beaucoup plus bornée, (N°. 24,) à cause du signe — qui oblige de lui donner la forme - - - $\left\{\frac{ddy}{(dx\sqrt{-1})(dx\sqrt{-1})}\right\} = \left\{\frac{ddy}{(dt\sqrt{-1})(dt\sqrt{-1})}\right\}$. La solution qu'on tire de cette dernière équation est à peu près semblable à celle que NEWTON a donnée d'un problême analogue, dans la 47e. proposition du second livre de ses Principes, et qui a paru peu satisfaisante aux géométres qui sont venus depuis ce grand homme. Oserois-je ajouter qu'ils n'en ont pas saisi le vrai sens?

L'explication que j'ai donnée du signe $\sqrt{-1}$ me paroît devoir applanir bien des difficultés.

Fig. 1.

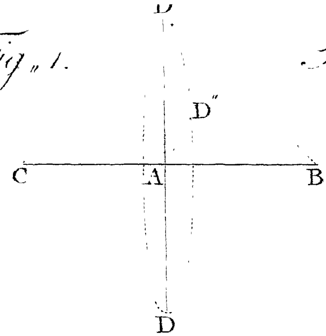


Fig. 2.

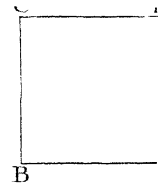


Fig. 4.

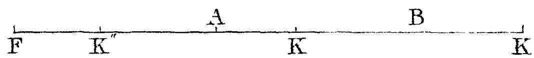
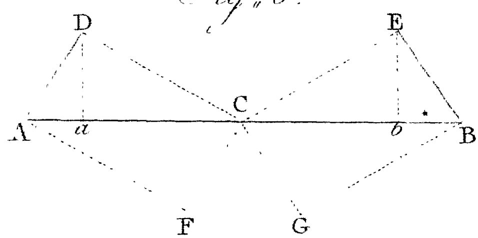


Fig. 6.



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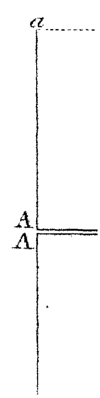


Fig. 8.

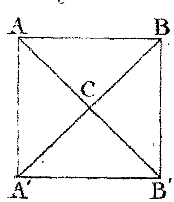
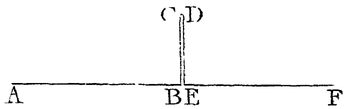
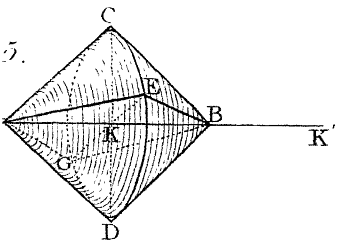


Fig. 3.



5.



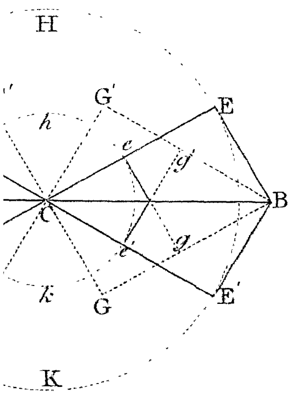


Fig. 10.

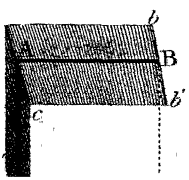


Fig. 11.

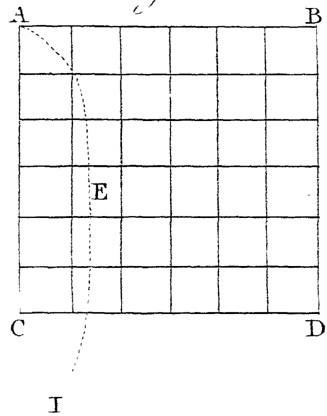
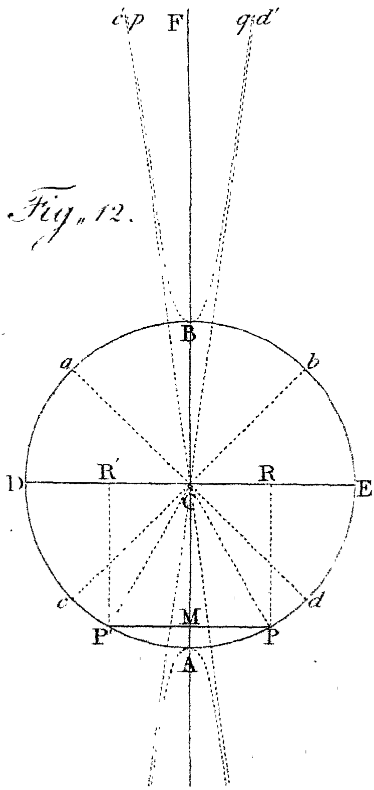


Fig. 12.



IV. *Chemical Experiments on Guaiacum. By Mr.* William Brande. *Communicated by* Charles Hatchett, *Esq. F.R.S.*

Read December 19, 1805.

Among the numerous substances which are comprehended under the name of resins, there is perhaps no one which possesses so many curious properties, as that now under consideration; and it is remarkable that no more attention has been paid to the subject, since many of the alterations which it undergoes when treated with different solvents, have been mentioned by various authors.

§ I.

Guaiacum has a green hue externally; is in some degree transparent; and breaks with a vitreous fracture.

When pulverised it is of a gray colour, but gradually becomes greenish on exposure to air.

It melts when heated, and diffuses at the same time a pungent aromatic odour.

It has when in powder a pleasant balsamic smell, but scarcely any taste, although when swallowed it excites a very powerful burning sensation in the throat.

Its specific gravity is 1.2289.

§ II.

1. When pulverised guaiacum is digested in a moderate heat with distilled water, an opaque solution is formed, which becomes clear on passing the whole through a filter.

The filtrated liquor is of a greenish-brown colour; it has a peculiar smell, and a sweetish taste.

It leaves on evaporation a brown substance, which is soluble in alcohol, nearly soluble in boiling water, and very little acted upon by sulphuric ether.

This solution was examined by the following re-agents.

Muriate of alumina occasioned a brown insoluble precipitate after some hours had elapsed.

Muriate of tin formed a brown flaky precipitate under the same circumstances.

Nitrate of silver gave a copious brown precipitate.

Suspecting the presence of lime in the solution, I added a few drops of oxalate of ammonia, when the liquid immediately became turbid, and deposited brown flakes, which, after having been treated with boiling alcohol, yielded traces of oxalate of lime.

These effects, therefore, indicate the presence of a substance in guaiacum, which possesses the properties of extract:* the action of the reagent is however somewhat modified, by a small quantity of lime which is also in solution.

One hundred grains of guaiacum yielded about nine grains of this impure extractive matter.

* By the term extract, I mean that substance, which by chemists is called the Extractive Principle of Vegetables. Vide THOMSON's Syst. of Chemistry, 2d edit. Vol. IV. p. 276.

2. Alcohol dissolves guaiacum with facility, leaving some extraneous matter, which generally amounts to about 5 *per cent.*

This solution is of a deep brown colour; the addition of water separates the resin, forming a milky fluid which passes the filter.

Acids produce the following changes:

A. Muriatic acid throws down an ash-coloured precipitate, which is not re-dissolved by heating the mixture. In this case the resin appears but little altered.

B. Liquid oxy-muriatic acid when poured into this solution, forms a precipitate of a very beautiful pale-blue colour, which may be preserved unaltered.

C. Sulphuric acid, when not added in too large a quantity, separates the resin of a pale green colour.

D. Acetic acid does not form any precipitate. This acid is indeed capable of dissolving most of the resins.

E. Nitric acid diluted with one-fourth of its weight of water, causes no precipitate till after the period of some hours. The liquid at first assumes a green colour, and if water be added at this period, a green precipitate may be obtained; the green colour soon changes to blue, (when by the same means a blue precipitate may be obtained;) it then becomes brown, and a brown precipitate spontaneously makes its appearance, the properties of which will be afterwards mentioned.

The changes of colour produced by nitric, and oxy-muriatic acids, in the alcoholic solution, are very remarkable, and I believe peculiar to guaiacum: there is moreover much reason to suppose that the above alterations in colour are occasioned

by oxygen.* It likewise appears from that which has been stated, that the blue and green oxides (if they may be so called by way of distinction) are soluble in the mixture of nitric acid and alcohol, while the brown precipitate is insoluble.

F. Alkalis do not form any precipitate when added to the solution of guaiacum in alcohol.

3. Guaiacum is less soluble in sulphuric ether than in alcohol; the properties of this solution nearly coincide with those just mentioned.

4. Muriatic acid dissolves a small portion of guaiacum, the solution assuming a deep brown colour; but if heat be applied, the resin melts into a blackish mass, preventing any farther action from taking place.

5. Sulphuric acid forms with guaiacum a deep red liquid, which, when fresh prepared, deposits a lilac coloured precipitate on the addition of water; a precipitate is also formed

* The following experiments appear to verify this supposition:

Fifty grains of freshly pulverised guaiacum were introduced into a glass jar containing 60 cubic inches of oxy-muriatic acid gas. The resin speedily assumed a brown colour, having passed through several shades of green and blue. Liquid ammonia was poured on this brown substance, while yet immersed in the acid; the whole became green; it therefore seemed thus to be deprived of part of the oxygen which it apparently had acquired by the preceding experiment. An equal portion of the same guaiacum was exposed under similar circumstances to the action of oxy-muriatic acid, excepting that the glass in which the experiment was made, was covered with a black varnish, and placed in a dark apartment. On examining the result of this experiment, the resin was found to have undergone precisely the same changes as when exposed to light. Ammonia had also the same effect.

Guaiacum was also exposed over mercury to oxygen gas; the resin assumed after some days the green colour which a longer exposure to the atmosphere produces: this change was likewise found by a second experiment to be effected without the presence of light.

by the alkalis. If heat be employed in forming this solution, the resin is speedily decomposed; and if the whole of the acid be evaporated, there remains a black coaly substance, together with some sulphate of lime.

6. Nitric acid appears to exert a more powerful action on guaiacum than on any of the resinous bodies.

100 grains of pure guaiacum previously reduced to powder, were cautiously added to two ounces of nitric acid, of the specific gravity of 1.39. The resin at first assumed a dark green colour, a violent effervescence was produced, attended with the emission of much nitrous gas, and the whole was dissolved without the assistance of heat, which is not the case with the resins in general, for when these bodies are thus treated with nitric acid, they are commonly converted into an orange-coloured porous mass.

The solution thus formed, yielded while recent, a brown precipitate with the alkalis, which was redissolved on the application of heat, forming a deep brown liquid.

Muriatic acid also separated the guaiacum from this solution, not however without having undergone some change.

Sulphuric acid caused no precipitate.

After this solution of guaiacum in nitric acid had remained undisturbed for some hours, a considerable proportion of crystallised oxalic acid was deposited.

When guaiacum was treated with dilute nitric acid, the results were somewhat different. A slight effervescence took place, and part of the resin was dissolved, the remainder being converted into a brown substance, resembling the precipitate obtained from the alcoholic solution as above mentioned. (2. E.)

This brown substance appears to be guaiacum, the properties of which are materially altered, by its combination with oxygen; and I am led to think that the changes of colour produced by nitric and oxy-muriatic acids, are the consequence of the different proportions of oxygen with which the guaiacum has been united; for we know that the colours of metallic, and many other bodies, are greatly influenced by the same cause.

The brown substance was separated by filtration; the filtrated liquor yielded yellow flocculent precipitates with the alkalis, and on examination was found to hold nitrate of lime in solution.

The undissolved portion was of a deep chocolate-brown colour. A similar substance may also be obtained, by evaporating the recent nitric solution to dryness, taking care not to apply too much heat towards the end of the process.

The substance obtained by either of these means, possesses the properties of a resin in greater perfection than guaiacum; it is equally soluble in alcohol and sulphuric ether, insoluble in water, &c.; but when burned it emits a peculiar smell, more resembling animal than vegetable bodies. If, however, fresh portions of nitric acid be added three or four times successively; or if a large quantity be employed to form the solution; the product obtained by evaporation is then of a very different nature; for it has lost all the characteristic properties of a resin, having become equally soluble in water and alcohol; the solution of it in this state having an astringent bitter taste.*

* Vide Mr. HATCHETT's two Papers on an artificial Substance which possesses the principal characteristic Properties of Tannin. Phil. Trans. 1805, p. 211, and 285.

7. Guaiacum is copiously soluble in the pure and carbonated alkalis, forming greenish-brown liquids.

Two ounces of a saturated solution of caustic potash took up rather more than 65 grains of the resin; the same quantity of liquid ammonia dissolved only 25 grains.

Nitric acid formed in these solutions a deep brown precipitate, the shades of which varied according to the quantity of acid which had been employed.

This precipitate was found on examination to possess the properties of that formed by nitric acid in the solution of guaiacum (2. E.) in alcohol.

Dilute sulphuric acid, when poured into any of the above alkaline solutions, formed a flesh-coloured curdy precipitate. Muriatic acid produced the same effect.

The two last mentioned precipitates differ from guaiacum, in being less acted upon by sulphuric ether and more soluble in boiling water, their properties therefore approach nearer to extract. Moreover, when these precipitates were redissolved in ammonia, and were again separated by muriatic acid, the above mentioned properties became more evident.

§ III.

100 grains of very pure guaiacum in powder, were put into a glass retort, to which the usual apparatus was adapted. The distillation was gradually performed on an open fire, until the bottom of the retort became red hot.

The following products were obtained:

	Grains.
Acidulated water - - - -	5.5
Thick brown oil, becoming turbid on cooling	24.5
Thin empyreumatic oil - - -	30.0
Coal remaining in the retort - - -	30.5
Mixed gases, consisting chiefly of carbonic acid and carbonated hydrogen - - -	9.0
	99.5

The coal, amounting to 30.5 grains, yielded on incineration 3 grains of lime. To discover whether any fixed alkali was present, 200 grains of the purest guaiacum (that in drops) were reduced to ashes; these were dissolved in muriatic acid, and precipitated by ammonia; the whole was then filtrated, and the clear liquor evaporated to dryness, but not any trace of a neutral salt with a basis of fixed alkali was perceptible.

§ IV.

From the action of different solvents on guaiacum, it appears, that although this substance possesses many properties in common with resinous bodies, it nevertheless differs from them in the following particulars:

1. By affording a portion of vegetable extract.

2. By the curious alterations which it undergoes when

subjected to the action of bodies, which readily communicate oxygen, such as nitric and oxy-muriatic acids; and the rapidity with which it dissolves in the former.

3. By being converted into a more perfect resin; in which respect guaiacum bears some resemblance to the green resin which constitutes the colouring matter of the leaves of trees, &c.*

4. By yielding oxalic acid.

5. By the quantity of charcoal and lime which are obtained from it when subjected to destructive distillation.

§ V.

From the whole therefore of the above mentioned properties, it evidently appears that guaiacum is a substance very different from those which are denominated resins, and that it is also different from all those which are enumerated amongst the balsams, gum resins, gums, and extracts: most probably it is a substance distinct in its nature from any of the above, in consequence of certain peculiarities in the proportions and chemical combination of its constituent elementary principles; but as this opinion may be thought not sufficiently supported by the facts which have been adduced, we may for the present be allowed to regard guaiacum as composed of a resin modified by the vegetable extractive

* This substance was found by PROUST to be insoluble in water, and soluble in alcohol. When treated with oxy-muriatic acid, it assumed the colour of a withered leaf, acquiring the resinous properties in greater perfection. Vide THOMSON's Syst. of Chemistry, 2d edit. Vol. IV. p. 318.

principle, and as such, perhaps the definition of it by the term of an *Extracto-Resin* may be adopted without impropriety.

P. S. I have observed that the action of oxygen on some of the other resinous bodies is very remarkable. It is well known that by digesting mastich in alcohol, a partial solution only is formed, and there remains an elastic substance, which is generally said to possess the properties of pure caoutchouc; it appears however to differ from this substance in becoming hard when dried by exposure to air. Moreover, I have remarked that the part of mastich which remains dissolved by alcohol, may be again precipitated by water, and, on examination, I found the precipitate to possess the properties of a pure resin: but when a stream of oxy-muriatic acid gas was made to pass through the solution, a tough elastic substance was thrown down, which became brittle when dried, and was soluble in boiling alcohol, but separated again as the solution cooled: its properties, therefore, somewhat approached to those of the original insoluble part.

V. *On the Direction of the Radicle and Germen during the Vegetation of Seeds. By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read January 9, 1806.

MY DEAR SIR,

IT can scarcely have escaped the notice of the most inattentive observer of vegetation, that in whatever position a seed is placed to germinate, its radicle invariably makes an effort to descend towards the centre of the earth, whilst the elongated germen takes a precisely opposite direction; and it has been proved by DU HAMEL* that if a seed, during its germination, be frequently inverted, the points both of the radicle and germen will return to the first direction. Some naturalists have supposed these opposite effects to be produced by gravitation; and it is not difficult to conceive that the same agent, by operating on bodies so differently organized as the radicle and germen of plants are, may occasion the one to descend and the other to ascend.

The hypothesis of these naturalists does not, however, appear to have been much strengthed by any facts they were able to adduce in support of it, nor much weakened by the arguments of their opponents; and therefore, as the phenomena

* *Physique des Arbres.*

observable during the conversion of a seed into a plant are amongst the most interesting that occur in vegetation, I commenced the experiments, an account of which I have now the honour to request you to lay before the Royal Society.

I conceived that if gravitation were the cause of the descent of the radicle, and of the ascent of the germen, it must act either by its immediate influence on the vegetable fibres and vessels during their formation, or on the motion and consequent distribution of the true sap afforded by the cotyledons: and as gravitation could produce these effects only whilst the seed remained at rest, and in the same position relative to the attraction of the earth, I imagined that its operation would become suspended by constant and rapid change of the position of the germinating seed, and that it might be counteracted by the agency of centrifugal force.

Having a strong rill of water passing through my garden, I constructed a small wheel similar to those used for grinding corn, adapting another wheel of a different construction, and formed of very slender pieces of wood, to the same axis. Round the circumference of the latter, which was eleven inches in diameter, numerous seeds of the garden bean, which had been soaked in water to produce their greatest degree of expansion, were bound, at short distances from each other. The radicles of these seeds were made to point in every direction, some towards the centre of the wheel, and others in the opposite direction; others as tangents to its curve, some pointing backwards, and others forwards, relative to its motion; and others pointing in opposite directions in lines parallel with the axis of the wheels. The whole was inclosed in a box, and secured by a lock, and a wire grate was placed

to prevent the ingress of any body capable of impeding the motion of the wheels.

The water being then admitted, the wheels performed something more than 150 revolutions in a minute; and the position of the seeds relative to the earth was of course as often perfectly inverted, within the same period of time; by which I conceive that the influence of gravitation must have been wholly suspended.

In a few days the seeds began to germinate, and as the truth of some of the opinions I had communicated to you, and of many others which I had long entertained, depended on the result of the experiment, I watched its progress with some anxiety, though not with much apprehension; and I had soon the pleasure to see that the radicles, in whatever direction they were protruded from the position of the seed, turned their points outwards from the circumference of the wheel, and in their subsequent growth receded nearly at right angles from its axis. The germens, on the contrary, took the opposite direction, and in a few days their points all met in the centre of the wheel. Three of these plants were suffered to remain on the wheel, and were secured to its spokes to prevent their being shaken off by its motion. The stems of these plants soon extended beyond the centre of the wheel: but the same cause, which first occasioned them to approach its axis, still operating, their points returned and met again at its centre.

The motion of the wheel being in this experiment vertical, the radicle and germen of every seed occupied, during a minute portion of time in each revolution, precisely the same position they would have assumed had the seeds vegetated at

rest; and as gravitation and centrifugal force also acted in lines parallel with the vertical motion and surface of the wheel, I conceived that some slight objections might be urged against the conclusions I felt inclined to draw. I therefore added to the machinery I have described another wheel, which moved horizontally over the vertical wheels; and to this, by means of multiplying wheels of different powers, I was enabled to give many different degrees of velocity. Round the circumference of the horizontal wheel, whose diameter was also eleven inches, seeds of the bean were bound as in the experiment, which I have already described, and it was then made to perform 250 revolutions in a minute. By the rapid motion of the water-wheel much water was thrown upwards on the horizontal wheel, part of which supplied the seeds upon it with moisture, and the remainder was dispersed, in a light and constant shower, over the seeds in the vertical wheel, and on others placed to vegetate at rest in different parts of the box.

Every seed on the horizontal wheel, though moving with great rapidity, necessarily retained the same position relative to the attraction of the earth; and therefore the operation of gravitation could not be suspended, though it might be counteracted, in a very considerable degree, by centrifugal force: and the difference, I had anticipated, between the effects of rapid vertical and horizontal motion soon became sufficiently obvious, The radicles pointed downwards about ten degrees below, and the germens as many degrees above, the horizontal line of the wheel's motion; centrifugal force having made both to deviate 80 degrees from the perpendicular direction each would have taken, had it vegetated at rest. Gradually

diminishing the rapidity of the motion of the horizontal wheel, the radicles descended more perpendicularly, and the germens grew more upright; and when it did not perform more than 80 revolutions in a minute, the radicle pointed about 45 degrees below, and the germen as much above, the horizontal line, the one always receding from, and the other approaching to, the axis of the wheel.

I would not, however, be understood to assert that the velocity of 250, or of 80 horizontal revolutions in a minute will always give accurately the degrees of depression and elevation of the radicle and germen which I have mentioned; for the rapidity of the motion of my wheels was sometimes diminished by the collection of fibres of conferva against the wire grate; which obstructed in some degree the passage of the water: and the machinery, having been the workmanship of myself and my gardener, can not be supposed to have moved with all the regularity it might have done, had it been made by a professional mechanic. But I conceive myself to have fully proved that the radicles of germinating seeds are made to descend, and their germens to ascend, by some external cause, and not by any power inherent in vegetable life: and I see little reason to doubt that gravitation is the principal, if not the only agent employed, in this case, by nature. I shall therefore endeavour to point out the means by which I conceive the same agent may produce effects so diametrically opposite to each other.

The radicle of a germinating seed (as many naturalists have observed) is increased in length only by new parts successively added to its apex or point, and not at all by any

general extension of parts already formed: and the new matter which is thus successively added unquestionably descends in a fluid state from the cotyledons.* On this fluid, and on the vegetable fibres and vessels whilst soft and flexible, and whilst the matter which composes them is changing from a fluid to a solid state, gravitation, I conceive, would operate sufficiently to give an inclination downwards to the point of the radicle; and as the radicle has been proved to be obedient to centrifugal force, it can scarcely be contended that its direction would remain uninfluenced by gravitation.

I have stated that the radicle is increased in length only by parts successively added to its point: the germen, on the contrary, elongates by a general extension of its parts previously organized; and its vessels and fibres appear to extend themselves in proportion to the quantity of nutriment they receive. If the motion and consequent distribution of the true sap be influenced by gravitation, it follows, that when the germen at its first emission, or subsequently, deviates from a perpendicular direction, the sap must accumulate on its under side: and I have found in a great variety of experiments on the seeds of the horse chesnut, the bean, and other plants, when vegetating at rest, that the vessels and fibres on the under side of the germen invariably elongate much more rapidly than those on its upper side; and thence it follows that the point of the germen must always turn upwards. And it has been proved that a similar increase of growth takes place on the external side of the germen when the sap

* See Phil. Trans. of 1805.

is impelled there by centrifugal force, as it is attracted by gravitation to its under side, when the seed germinates at rest.

This increased elongation of the fibres and vessels of the under side is not confined to the germens, nor even to the annual shoots of trees, but occurs and produces the most extensive effects in the subsequent growth of their trunks and branches. The immediate effect of gravitation is certainly to occasion the further depression of every branch, which extends horizontally from the trunk of the tree; and, when a young tree inclines to either side, to increase that inclination: but it at the same time, attracts the sap to the under side, and thus occasions an increased longitudinal extension of the substance of the new wood on that side.* The depression of the lateral branch is thus prevented; and it is even enabled to raise itself above its natural level, when the branches above it are removed; and the young tree, by the same means, becomes more upright, in direct opposition to the immediate action of gravitation: nature, as usual, executing the most important operations by the most simple means.

I could adduce many more facts in support of the preceding deductions, but those I have stated, I conceive to be sufficiently conclusive. It has however been objected by Du Hamel, (and the greatest deference is always due to his opinions,) that gravitation could have little influence on the direction of the germen, were it in the first instance protruded, or were it subsequently inverted, and made to

* This effect does not appear to be produced in what are called weeping trees; the cause of which I have endeavoured to point out in a former Memoir. Phil. Trans. 1804.

point perpendicularly downwards. To enable myself to answer this objection, I made many experiments on seeds of the horse chesnut, and of the bean, in the box I have already described; and as the seeds there were suspended out of the earth, I could regularly watch the progress of every effort made by the radicle and germen to change their positions. The extremity of the radicle of the bean, when made to point perpendicularly upwards, generally formed a considerable curvature within three or four hours, when the weather was warm. The germen was more sluggish; but it rarely or never failed to change its direction in the course of twenty-four hours; and all my efforts to make it grow downwards, by slightly changing its direction, were invariably abortive.

Another, and apparently a more weighty, objection to the preceding hypothesis, (if applied to the subsequent growth and forms of trees,) arises from the facts that few of their branches rise perpendicularly upwards, and that their roots always spread horizontally; but this objection I think may be readily answered.

The luxuriant shoots of trees, which abound in sap, in whatever direction they are first protruded, almost uniformly turn upwards, and endeavour to acquire a perpendicular direction; and to this their points will immediately return, if they are bent downwards during any period of their growth; their curvature upwards being occasioned by an increased extension of the fibres and vessels of their under sides, as in the elongated germens of seeds. The more feeble and slender shoots of the same trees will, on the contrary, grow in almost every direction, probably because their fibres, being more dry, and their vessels less amply supplied with sap, they are

less affected by gravitation. Their points, however, generally shew an inclination to turn upwards; but the operation of light, in this case, has been proved by BONNET* to be very considerable.

The radicle tapers rapidly, as it descends into the earth, and its lower part is much compressed by the greater solidity of the mould into which it penetrates. The true sap also continues to descend from the cotyledons and leaves, and occasions a continued increase of the growth of the upper parts of the radicle, and this growth is subsequently augmented by the effects of motion, when the germen has risen above the ground. The true sap is therefore necessarily obstructed in its descent; numerous lateral roots are generated, into which a portion of the descending sap enters. The substance of these roots, like that of the slender horizontal branches, is much less succulent than that of the radicle first emitted, and they are in consequence less obedient to gravitation: and therefore meeting less resistance from the superficial soil, than from that beneath it, they extend horizontally in every direction, growing with most rapidity, and producing the greatest number of ramifications, wherever they find most warmth, and a soil best adapted to nourish the tree. As these horizontal, or lateral, roots surround the base of the tree on every side, the true sap descending down its bark, enters almost exclusively into them, and the first perpendicular root, having executed its office of securing moisture to the plant, whilst young, is thus deprived of proper nutriment, and, ceasing almost wholly to grow, becomes of no importance to the tree. The tap root of the oak, about which so

* *Recherches sur l'Usage des Feuilles dans les Plantes.*

much has been written, will possibly be adduced as an exception; but having attentively examined at least 20,000 trees of this species, many of which had grown in some of the deepest and most favourable soils of England, and never having found a single tree possessing a tap root, I must be allowed to doubt that one ever existed.

As trees possess the power to turn the upper surfaces of their leaves, and the points of their shoots to the light, and their tendrils in any direction to attach themselves to contiguous objects, it may be suspected that their lateral roots are by some means directed to any soil in their vicinity which is best calculated to nourish the plant, to which they belong; and it is well known that much the greater part of the roots of an aquatic plant, which has grown in a dry soil, on the margin of a lake or river, have been found to point to the water; whilst those of another species of tree which thrives best in a dry soil, have been ascertained to take an opposite direction: but the result of some experiments I have made is not favourable to this hypothesis, and I am rather inclined to believe that the roots disperse themselves in every direction, and only become most numerous where they find most employment, and a soil best adapted to the species of plant. My experiments have not, however, been sufficiently varied, or numerous, to decide this question, which I propose to make the subject of future investigation.

I am, &c.

T. A. KNIGHT.

Elton, Nov. 22, 1805.

VI. *A third Series of Experiments on an artificial Substance, which possesses the principal characteristic Properties of Tannin; with some Remarks on Coal. By* Charles Hatchett, *Esq. F. R. S.*

Read January 16, 1806.

§ I.

In my former Papers upon this subject, some account has been given of the effects produced by sulphuric acid upon turpentine, resin, and camphor; and I shall now state the results of other experiments made with the same acid upon a great number of the resins, balsams, gum resins, and gums, the greater part of which, afforded that modification of the artificial tanning substance, which for the sake of distinction, I have in the preceding papers denominated the third variety.

The process was simple digestion in sulphuric acid, after which, the residuum was well edulcorated, and was then digested in alcohol. This was separated by distillation, the dry substance which remained was infused in cold distilled water, and the portion dissolved, was examined by solution of isinglass, muriate of tin, acetite of lead, and sulphate of iron.

Much sulphureous acid, carbonic acid, several of the vegetable acids, particularly benzoic acid, (when the balsams were employed,) and apparently water, were produced during the operation; but in this Paper I shall only notice two of the products, namely, the tanning substance and the coal.

The sulphuric acid almost immediately dissolved the resins, and formed transparent brown solutions, which progressively became black.

The same effect was produced on most of the other substances, but the solutions of the balsams and of guaiacum were at first of a deep crimson, slightly inclining to brown.

Caoutchouc and elastic bitumen were not dissolved, but after having been digested for more than two months, were only superficially carbonized.

The gums and the saccharine substances required many evaporations and filtrations before the whole of their carbonaceous residua could be obtained.

These were the principal effects observed during the experiments, and I have stated them in this manner, that tedious and useless repetitions may be avoided.

§ II.

Turpentine, common resin, elemi, tacamahac, mastich, copaiba, copal, camphor, benzoin, balsam of Tolu, balsam of Peru, asa fœtida, and amber, yielded an abundance of the tanning substance.

Oil of turpentine also afforded much of it; asphaltum yielded a small portion; some slight traces of it were even obtained from gum arabic and tragacanth; but none was produced by guaiacum, dragon's blood, myrrh, gum ammoniac, olibanum, gamboge, caoutchouc, elastic bitumen, liquorice, and manna. I am persuaded, however, that many of these would have afforded the tanning substance had not the digestion been of too long a duration.

Olive oil was partly converted into the above mentioned

substance, and also linseed oil, wax, and animal fat; but the three last appear to merit some attention.

Linseed Oil.

This oil with sulphuric acid very soon formed a thick blackish-brown liquid, which after being long digested in a sand-bath, was still partly soluble in cold water, and passed the filter. This solution precipitated gelatine; the residuum was a tough black substance, which became hard on exposure to air. A great part was soluble in alcohol, and formed a brown liquid, which became turbid by the addition of water. When this was evaporated, a brown substance remained, which was partially dissolved by cold water, and the solution thus formed, was rendered turbid by gelatine.

The undissolved portion left by the alcohol, was of a blackish-brown; it was soft and tenacious, and appeared to retain many of the properties of an inspissated fat oil.

Bleached Wax.

That which was employed in this experiment, was the white wax of the shops, which is sold in the form of small round cakes. It formed with sulphuric acid a thick black magma, and was not acted upon by cold distilled water when washed with it upon a filter. Upon being digested with alcohol in a sand-bath, a brownish solution was formed, which upon cooling became very turbid, and appeared as if filled with a white flocculent substance. The same operation was repeated with different portions of alcohol until this ceased to act. The whole of the solutions in alcohol were

then mixed, a large quantity of distilled water was added, and the alcohol was separated by distillation.

On the surface of the remaining liquor, when cold, a white crust was formed, which being separated, was found to possess the properties of spermaceti, and weighed 18 grains. The filtrated liquor was then evaporated to a small quantity, became of a pale brown colour, and was rendered turbid by solution of isinglass.

Animal Fat.

This experiment was made upon the kidney fat of veal, but I cannot take upon me to assert that the results would have been the same with every kind of fat. 100 grains of it with one ounce of concentrated sulphuric acid, after some time, formed a blackish soft mass; a second ounce of sulphuric acid was then added, and the whole was digested and occasionally heated during nearly three months. Six ounces of distilled water were poured upon the black pulpy mass, and formed a thick uniform liquid, which, after digestion for six or seven days, was when cold filtrated. The liquor which passed was of a brown colour, and upon evaporation became black, leaving a considerable portion of a blackish substance upon the filter, which was added to that which had been collected by the first filtration. The whole was washed with cold water, which passed colourless. Boiling water was then poured upon the filter, by which a considerable portion was rapidly dissolved, and a brownish-black solution was formed, which copiously precipitated gelatine.

The residuum on the filter was then dried, and being col-

lected, was digested in alcohol, which dissolved the greater part.

The solution in alcohol was filtrated, but (apparently by the effect of air) a considerable deposit was formed on the filter, which was again dissolved by alcohol. Water rendered the solution turbid, and a black light flaky substance, which weighed 41 grains, remained upon the filter. The filtrated liquor was then evaporated, and left a grayish-black substance, which weighed 30 grains. This last substance was highly inflammable, and when burned, emitted a very peculiar odour, resembling partly that of fat and partly that of asphaltum. It easily melted, and also immediately dissolved in cold alcohol, from which, like the resinous substances, it was precipitated by water.

The black light flaky residuum, which weighed 41 grains, was found to consist partly of the substance above mentioned and partly of coal, but the proportion of this last was not ascertained.

Coagulated albumen and prepared muscular fibre were also separately exposed to the action of sulphuric acid in the manner above described, but did not afford any substance by which gelatine could be precipitated, coal being the only product which remained.

Almost every one of the bodies which have been employed in these experiments, seem to be in some measure different in respect to the progressive effects produced upon them by sulphuric acid; and all other circumstances being similar, there appears to be a certain period of the process when the production of the tanning substance has arrived at its maximum, after which, a gradual diminution of it takes place, and

at length total destruction. These effects are produced at different periods, according to the substance which may be the subject of the experiment, and therefore it is impossible at present to state the utmost quantity of the tanning substance which, under equal circumstances, may be obtained from each of the resins, balsams, &c.

The tanning substance appears to be always the same, whether obtained from turpentine, or common resin, or from the balsams, or from asa fœtida, or camphor, or indeed from any of the bodies which have been enumerated; its effects on the different reagents are similar; by the addition of a small portion of nitric acid, and subsequent evaporation, it is converted into that which I have called the first variety; or if digested with sulphuric acid, it is speedily destroyed, and becomes mere coal. In the latter case, therefore, the same agent which at first produced it becomes at length the cause of its destruction, and thus we find, that although a tanning substance may be obtained from resinous and other bodies by means of sulphuric and by nitric acid, yet in the former case the product is variable, and is formed at or about the mean period of the operation, whilst the latter is an ultimate and invariable effect, beyond which, no apparent change can be produced by any continuation of the process.*

§ III.

I have already stated, that caoutchouc, and elastic bitumen, were only superficially acted upon when digested for a very

* In the former Papers upon this subject I have observed, that the tanning substance produced by sulphuric acid, is very inferior in energy to that, which is formed by nitric acid.

long time in sulphuric acid; and it is remarkable, that these substances, which in their external characters so much resemble each other, should be similar in their habits when exposed to the effects of this acid; for, unlike the resins and most of the other bodies which were subjected to the preceding experiments, and which were almost immediately dissolved when the acid was poured upon them, these on the contrary remained undissolved, and only became partially carbonized on their surfaces. Even nitric acid does not so rapidly effect a change in the elastic bitumen as it does when applied to the other bituminous substances.

1.

100 grains of pure soft elastic bitumen were digested during three weeks in one ounce of nitric acid, diluted with an equal quantity of water; a tough and slightly elastic orange-coloured mass then remained. Another ounce of the acid, not diluted, was poured upon this mass, and the digestion was continued until the whole was evaporated. The residuum was tenacious, and of the colour above mentioned. Water partially dissolved it, and formed a deep yellow liquid, which copiously precipitated gelatine, and possessed the other properties of the tanning substance which is produced from the resins, &c. by nitric acid.

An orange-coloured mass still remained, which was speedily dissolved by alcohol, and was precipitated from it by a large addition of water.

This substance in many of its properties resembled the resins, but in others, seemed to approach those which characterize the vegetable extractive matter. It appeared to

be similar to that which has been cursorily mentioned in my first Paper, and which was obtained from many of the pit-coals and bitumens when treated with nitric acid. I have since paid more attention to this substance during the following experiments:

Kilkenny coal was digested with nitric acid, and progressively, although with difficulty, was converted into that variety of the tanning substance which has so often been mentioned. Similar experiments were made on the same sort of coal from Wales, which was given to me by my friend Mr. TENNANT, as well as upon a coal sent to me by Professor WOODHOUSE, which was from Pensylvania, and is there called Leigh high coal. All of these were converted into the tanning substance, but they did not yield any product similar to that obtained from the elastic bitumen.

The contrary however happened when the common pit-coal, or Cannel coal, or asphaltum, were employed. For when these were treated in the way which has been described, and when the digestion was not too long continued, then I obtained from 100 grains of each of the above substances (after the separation of the tanning matter) a residuum as follows:

From 100 grains of the common Newcastle coal 9 grains.
From 100 grains of Cannel coal - - - 36 grains.
From 100 grains of pure asphaltum - - 37 grains.

The substances thus obtained, were very similar in their external characters, being of a pale brown, approaching to Spanish snuff colour; their internal fracture was dark brown, with a considerable degree of resinous lustre. When exposed to heat they did not easily melt, but as soon as inflamed, they emitted a resinous odour mixed with that of fat oil, and pro-

duced a very light coal, much exceeding the bulk of the original substance.

Alcohol completely dissolved them, and if water in a large proportion was added to a saturated solution, a precipitate was obtained, but after each precipitation, a portion always remained dissolved by the water, which acted upon the different reagents in a manner similar to the solutions of vegetable extractive matter. The flavour was also bitter, and in some degree aromatic, so that the residua, whether obtained from pit-coal, from Cannel coal, or from asphaltum, seemed to possess properties intermediate between those of resin, and those of the vegetable extractive substance. They appeared however, to be removed only by a very few degrees from the tanning substance; for if digested in a small quantity of nitric acid, and subsequently evaporated, they were immediately converted into it; or if digested with sulphuric acid, they speedily became reduced to coal.

§ IV.

In the 5th Section of my second Paper, some remarks were made on the decoctions obtained from vegetable substances which had been previously roasted; and although (excepting one instance) these decoctions did not afford any permanent precipitate with gelatine, yet I have there stated, that I did not think it right to conclude, that similar decoctions made under certain circumstances, might not occasionally possess those properties which characterize the tanning substances. Moreover I also observed in the same Paper, that all of those decoctions, upon the addition of a small portion of nitric acid and subsequent evaporation, became converted into that variety of tanning matter which is produced by the action of nitric

acid upon carbonaceous substances. I have since extended these experiments, and shall here give some account of them.

1.

200 grains of the fresh peels of horse chesnuts were digested for about 12 hours in three ounces of distilled water. The liquor was of a pale brown, and formed a slight pale brown precipitate when solution of isinglass was added to it.

2.

200 grains of the same peels were moderately roasted, and being afterwards digested with three ounces of water, formed a dark brown decoction, which was not rendered turbid by gelatine.

3.

The above mentioned roasted peels, after the termination of the preceding experiment, were added to the remainder of the filtrated liquor. A quarter of an ounce of nitric acid was poured upon the whole, which was then digested and evaporated to dryness. The mass was afterwards infused in water, and a dark reddish-brown liquid was obtained, which copiously precipitated solution of isinglass.

4.

200 grains of horse chesnuts, from which the peels employed in the former experiments had been taken, were bruised, and were digested with three ounces of water. The liquor was turbid, and of a pale red colour. It was filtrated, and some solution of isinglass was added, but without any effect.

5.

200 grains of the same horse chesnuts were moderately roasted, and being treated as above described with water, yielded a dark brown decoction which was not rendered turbid by isinglass.

6.

The horse chesnuts, which had been employed in the preceding experiment with the remaining liquor, were digested with a quarter of an ounce of nitric acid until the whole was become dry. Water was then poured upon it, was digested, and a dark brown liquid was formed, which afforded a considerable precipitate by the addition of solution of isinglass.

From these experiments it appears, that the small portion of tannin which the horse chesnut peels originally contained, was destroyed by the process of roasting; that the brown decoction subsequently obtained from the roasted peels and from the horse chesnuts, did not act upon gelatine; but that these were speedily converted into the artificial tanning substance, by the addition of a small portion of nitric acid and subsequent evaporation.

The first preparations of the artificial tanning substance which have been mentioned in the former Papers, were made from coal of different descriptions digested with nitric acid, and as similar products have been obtained by the same acid from various decoctions of roasted vegetable substances, there cannot be any doubt, that vegetable bodies when roasted, yield solutions by digestion in water, which essentially consist of carbon approaching to the state of coal, although not absolutely converted into it, for if so, all solubility in water would cease.

But coal is apparently nothing more than carbon oxidized

to a certain degree, and may be formed by the humid as well as by the dry way.

Examples have been already stated respecting operations in which sulphuric acid has produced this effect, but the same likewise appears to be produced with some modifications, whenever vegetable matter undergoes the putrefactive process; for when this takes place, as in dunghills, &c. a large proportion of the carbon of the original vegetable substances appears to be combined with oxygen sufficient to communicate to it many of the properties of coal, whilst the compound nevertheless is capable of being dissolved by water with the most perfect facility.

It must not however be understood that by this process all the other elementary principles are separated, so that only the carbon remains combined with oxygen, but merely, that the other principles are so far diminished, that these, namely, carbon and oxygen, predominate in a state approaching to coal, although soluble in water.

Such solutions, I have every reason to believe, are nearly similar to those afforded by vegetable substances which have been previously roasted, and although I have examined but a few of them, yet I shall relate some experiments which I have lately made on the peels of walnuts.

It is well known that when these are kept in small heaps for a short time, they become soft, and break down into a black mass, which affords a brownish-black liquor. On these I therefore made the following experiments:

1.

About one ounce of walnut peels, which were become soft and black, was digested in water.

A dark brown liquor was thus formed, and being filtrated,

was examined by a solution of isinglass, but not any apparent effect was produced.

2.

On an equal quantity of the walnut peels, in the same soft black state, a small portion of nitric acid was poured, and after being digested for about five hours, the whole was evaporated to dryness. The residuum was of a brownish orange colour, and yielded a similar coloured solution to water when digested with it. This was filtrated, and upon the addition of solution of isinglass, became turbid, and deposited a tough precipitate, which was not dissolved by boiling water.

3.

Another portion of the walnut peels was moderately roasted, and was then digested in water; the brown solution was filtrated, and formed a slight precipitate with gelatine.

4.

On the residuum of the last experiment, a small quantity of nitric acid was poured, some water was then added, the whole was digested during about five hours, and until it became perfectly dry.

Water formed with this a brown liquor, which yielded a very abundant precipitate by the addition of dissolved isinglass.

Upon these experiments we may remark, that the solution in the first instance contained carbon in a state approaching to coal, for when treated with nitric acid in the second experiment, a portion (although small) was produced of the same tanning substance which is formed from the different kinds of coal by nitric acid.

The third experiment appears to shew, that a small quantity of a substance approaching to tannin was produced by the simple process of roasting; and the fourth experiment corroborates those already described, in which, the artificial tanning matter was copiously produced, whenever roasted vegetable substances were treated with nitric acid.

In respect to vegetable substances, especially those which contain tannin, I shall here relate a few other experiments.

It has been remarked in my second Paper, (p. 288,) that the tannin of galls was immediately destroyed by nitric acid. Since that time, I have made the following additional experiments:

1.

100 grains of galls reduced to powder were infused with four ounces of water, and part of the infusion upon the addition of solution of isinglass afforded (as usual) a copious precipitate of a brownish-white colour.

A quarter of an ounce of nitric acid was added to one ounce of the above infusion, which then, was not in any manner affected by the dissolved isinglass.

2.

100 grains of the same galls were slightly roasted, and being digested with four ounces of water, formed a brown liquor, which was filtrated.

Solution of isinglass was then added to a part of the above liquor, and produced a precipitate not very unlike the former, but much less in quantity.

After this, a quarter of an ounce of nitric acid was added to one ounce of the same liquor, and some dissolved isinglass

was subsequently poured into it, by which it was rendered turbid, and a small portion of a dark brown precipitate was produced, resembling that which is commonly afforded by the artificial tanning substance.

3.

The remainder of the above mentioned liquor, with the residuum of the roasted galls, were digested with a quarter of an ounce of nitric acid until the whole had become dry. Water was then poured upon it, and formed a dark brown solution, which yielded a copious brown precipitate by the addition of dissolved isinglass.

From these experiments on galls it appears, that the natural tannin contained in them is destroyed by nitric acid; that the tannin is also diminished, (and I may add,) is ultimately destroyed by the process of roasting; that when galls have not been so far roasted as to destroy the whole of the tannin, then the remainder of this seems to be destroyed by the addition of nitric acid, whilst at the same time a small portion of the artificial tanning substance is produced; and that this last is always plentifully afforded by roasted galls when digested with nitric acid, similar to other vegetable bodies when thus treated.

These remarks are also partly confirmed by the following experiments upon oak bark.

1.

200 grains of oak bark, reduced into very small fragments, were infused in about four ounces of water, after which the infusion was examined by dissolved isinglass, and yielded a considerable precipitate.

2.

200 grains of the same sort of bark were slightly roasted, and afterwards digested in water; a much darker coloured liquor was obtained than in the former case; but although it afforded precipitates by the addition of muriate of tin, acetite of lead, and sulphate of iron, yet not the smallest effect was produced by solution of isinglass.

3.

The residuum, with the remaining part of the above mentioned liquor, was then digested with a small portion of nitric acid; this was completely evaporated, and a brown solution was formed by water, which abundantly precipitated gelatine.

4.

One ounce of oak bark, reduced into very small fragments, was repeatedly digested in different portions of water until the whole of its tannin was extracted. The residuum or exhausted bark (as it is called by the tanners) was dried, and was afterwards moderately roasted. It was then moistened with diluted nitric acid, which was evaporated in a heat not much exceeding 300° until the bark was become perfectly dry. This was digested in water, and speedily formed a yellowish-brown liquor, which abundantly precipitated gelatine.

5.

The bark, which after being exhausted of its natural tannin, had thus afforded the artificial tanning substance, was repeatedly treated with water until the whole of this last was extracted. The bark was then again slightly roasted, was again

moistened with nitric acid, and was gently heated and dried as before. Water being poured on it and digested, formed a brown solution, which copiously precipitated gelatine.

6.

The whole of the artificial tanning substance was extracted by different portions of water, and the remainder of the bark thus exhausted, was again treated in the manner above described, and again afforded a considerable quantity of the tanning substance, so that these processes evidently might have been continued until the whole of the bark had been converted into it.

This might also have been accomplished, if in the first instance, the exhausted bark had been converted into charcoal, and digested in nitric acid, as described in my first Paper; but then, the effects would have been more slowly produced, and much more nitric acid would have been consumed. I am now therefore fully convinced, not only by the results of the experiments related in this Paper, but also by many others which it would have been superfluous to have stated, that the most speedy and most economical of all the processes which I have described, is that of treating roasted vegetable substances in the way which has been mentioned, and considering that all refuse vegetable matter may be thus converted into a tanning substance by means the most simple, and without any expensive apparatus, I cannot help entertaining much hope, that eventually this discovery will be productive of some real public advantage.

§ V.

In my first Paper I have remarked, that I suspected the tannin of the peat moors to have been produced during the imperfect carbonization of the original vegetable substances. Whether this has been the case, or whether the tannin has at times been afforded by heath and other vegetables growing upon or near the peat, still appears to me to be uncertain; but whatever may be the origin, I never have yet been able to detect any tanning substance in peat, although I have examined a considerable number of varieties, some from Berkshire, and many from Lancashire, which were obligingly sent to me for this purpose by my friend JOHN WALKER, Esq. F. R. S. Mr. JAMESON has also made the same observation,* so that there cannot be any doubt (whatever the origin of the tanning matter may have been) that it has speedily been extracted and drained from the substances which at first contained it.

This effect is a natural consequence of the great facility with which tannin is dissolved by water, and extends even to the most solid vegetable bodies; I shall here give an example.

In the Philosophical Transactions for 1799, Dr. CORREA DE SERRA has given an account of a submarine forest at Sutton, on the coast of Lincolnshire, where submerged vegetables are found in great abundance, including trees of different descriptions, especially birch, fir, and oak. At the time when I was engaged in those experiments on the Bovey coal, and other substances of a similar nature, which have been printed in the Philosophical Transactions for 1804, Sir JOSEPH BANKS

* An Outline of the Mineralogy of the Shetland Islands, &c. 8vo. edition, p. 174.

had the goodness to send me a piece of the oak, which was perfect in all of its vegetable characters, and did not appear to have suffered any change excepting, that it was harder, and of a darker colour than recent oak wood. From some experiments which I then made, I found, that after incineration it afforded potash, similar to the recent wood, and contrary to substances like the Bovey coal, which retain the vegetable external characters, although imperfectly converted into coal.*

In the course of my experiments on tannin, I reduced about an ounce of this submerged oak into shavings, and digested them in water. A brown decoction was formed, which with muriate of tin afforded a pale brown precipitate; with acetite of lead, a precipitate of a deeper brown; with sulphate of iron, a copious brownish-black precipitate; but with solution of isinglass not any effect was produced.

The tannin of this oak wood, had therefore either been separated by solution, or had been decomposed; so that the only substance which remained capable of being dissolved by water, was the extractive matter. This last, in the present case, was most probably the original extractive matter of the oak, but in some other instances, (such, for example, as that which was found in the alder leaves contained in the Iceland schistus,†) I am much inclined to believe, that an extractive substance of secondary formation, if I may be permitted to employ such a term, is produced during the process of carbonization. If a substance, therefore, so compact and solid as oak timber can by long submersion be deprived of its tannin, it naturally follows that the same effect must be more speedily produced by the action of water on the smaller vegetable

* Phil. Trans. for 1804, p. 399. † Ibid. p. 391.

bodies, which present an extensive surface, and also on porous and bibulous substances such as peat.

But although peat, as I have already observed, does not contain any tannin, yet the imperfect carbonization which it has undergone, renders it like the roasted ligneous bodies, peculiarly susceptible of being converted into the artificial tanning substance when exposed to the action of nitric acid. It would be useless to enter into a detail of the different experiments which I have made upon it, as they were similar to those already related, and I shall therefore only here state, that when seven ounces of well dried peat had been twice moistened, and digested with diluted nitric acid, (to the amount of rather more than two ounces,) and subsequently dried, I obtained by water a solution of the artificial tanning substance, which when evaporated to dryness weighed two ounces. I am convinced, that much more might have been obtained from the residuum of the peat, had I thought proper to have repeated the operation; and I am also certain, that less nitric acid would have been sufficient, had the process been conducted in close vessels, and with other economical precautions, which at that time, were for the sake of expedition and convenience omitted.

§ VI.

It has been generally stated, even by modern chemists, that the acids act but little, if at all, upon resinous substances.

The contrary has however been proved, not only in the three Papers upon the present subject, but also in some others which I have formerly had the honour to lay before this learned Society.

In my experiments on lac, printed in the Phil. Trans. for 1804, p. 208, I have particularly endeavoured to shew, how powerfully the acetic acid acts upon resin, gluten, and some other substances; so that it may justly be regarded, as a valuable agent in the chemical analysis of vegetable bodies. In this point of view, it is as a solvent to be the more highly appreciated, because it appears to dissolve the resins, &c. without affecting their respective qualities, and thus by proper precipitants, these substances may be separated from it pure and unaltered.

I am induced therefore to consider acetic acid to be the true acid solvent of the resinous substances, as it dissolves them speedily, without producing any apparent subsequent change in their natural properties.

Sulphuric acid also, almost immediately dissolves the resins, balsams, &c. and forms transparent brown or sometimes crimson solutions, the latter colour being most commonly characteristic of the balsams.

These solutions, however, are different from those made in the acetic acid, by not being permanent, for from the moment when the solution is completed, progressive alterations appear to be produced in the body which is dissolved; thus turpentine is almost immediately converted into resin, then into the third variety of the tanning substance, and lastly into coal.

Without being under the necessity of adducing other examples, we may therefore state sulphuric acid to be a solvent of the resinous substances, but which continues afterwards to act on their principles, so as to decompose them, coal being the ultimate product.

Nitric acid, as I have shewn in the course of these Papers,

and likewise on some former occasions, dissolves the resins, but the progress of its effects seems to be conversely that of sulphuric acid; in the latter case, solution precedes decomposition; but when nitric acid is employed, decomposition to a certain degree precedes solution; for it at first converts the resins into a pale orange-coloured brittle porous substance, then into a product, which apparently possesses the intermediate characters of vegetable extractive matter and of resin, and lastly, this is converted into the first variety of the tanning substance, beyond which I have not been able to effect any change.

As coal therefore appears to be the ultimate effect produced by sulphuric acid upon the resinous bodies, so does the first variety of the tanning substance seem to be the terminating product afforded by the same when acted upon by nitric acid. This effect of nitric acid has been already amply discussed, neither does it appear necessary that I should here repeat the remarks which have been made on some of the simultaneous products, such as the vegetable acids; but amongst the effects produced by sulphuric acid, the coal which is formed seems to merit some attention.

§ VII.

After the tanning substance and the other products had been obtained from the resins, balsams, &c. which have been mentioned in the beginning of this Paper, the following proportions of coal remained.*

* The weight of the coal obtained from each of the above mentioned substances, was estimated after the complete separation of every other product, and after the moisture had been expelled by a red heat, in close vessels.

		Coal.
100 grains of	Copal - - - -	67 grains.
———	Mastich - - -	66
———	Balsam of Peru - -	64
———	Elemi - - - -	63
———	Tacamahac - - -	62
———	Guaiacum - - -	58
———	Gum ammoniac - -	58
———	Amber - - -	56
———	Olive oil - - -	55
———	Balsam of Tolu - -	54
———	Asa fœtida - - -	51
———	Wax - - - -	50
———	Dragon's blood - -	48
———	Benzoin - - -	48
———	Olibanum - - -	44
———	Myrrh - - -	40
———	Asphaltum - - -	40
———	Gamboge - - -	31
———	Elastic bitumen - -	31
———	Gum arabic - - -	29
———	Liquorice - - -	25
———	Manna - - - -	25
———	Tragacanth - -	22
———	Caoutchouc - - -	12*

The coal obtained from the resinous bodies by means of sulphuric acid, is in a much greater proportion, than when

* Caoutchouc and elastic bitumen were only superficially carbonized by the sulphuric acid, so that the proportion of coal as above stated, is considerably less than that, which in reality might have been obtained from them.

equal quantities of those substances are exposed to simple distillation.

For, (as I have stated in my first Paper,) 100 grains of common resin by the humid process afforded 43 of coal, which after a red heat still weighed 30 grains.

But the same quantity of resin by distillation, only yielded $\frac{3}{4}$ of a grain of coal.

100 grains of mastich, by the first method, afforded 66 grains of coal.

100 grains of the same mastich only gave $4\frac{1}{2}$ grains of coal when simply distilled.

And 100 grains of amber, when treated with sulphuric acid, yielded 56 grains of coal.

But from 100 grains of the same amber when distilled, only $3\frac{1}{2}$ grains could be obtained.

Many other examples might be adduced, but these appear to be sufficient; and I must here observe, that the case is very different in respect to the gums, for the difference between the proportions of coal obtained from them by the humid and dry ways is not very considerable, although it is always the greatest in the former process, when conducted with precaution. Moreover it is to be remarked, that in either process, variations in the quantity of coal are produced by difference of temperature, by the figure and size of the vessels, and many other circumstances.

But it is not only in the proportion, that there is so great a difference between the coal obtained from the resinous substances by the humid way or by fire, for the quality is also most commonly different; and this not only applies to the resins but also to ligneous matter.

The coal obtained by the humid process from many of the resins, was shining, hard, and occasionally iridescent. Few of the coals obtained from the same bodies by fire had any of these properties. The combustion of the former was slow in the manner of some of the mineral coals, whilst on the contrary the latter were speedily consumed like charcoal. This difference I was at first inclined to attribute to a small portion of the acid which might not have been completely separated, and I therefore purposely made some experiments which convinced me that this was not the case.

Having remarked this difference in the coals afforded by the resins, I was desirous to make some comparative experiments on wood, and for this purpose I selected oak.

1.

On 480 grains of oak sawdust I poured two ounces of sulphuric acid diluted with six ounces of water, and placed the matrass on a sand-bath, where it remained from the beginning of last June to the end of September. During this time, the sand-bath had very seldom been heated, but the vessel was occasionally shaken.

At the end of the period above mentioned, six ounces of boiling water were added, and the whole being poured upon a filter, was repeatedly washed, and was afterwards dried on a sand-bath in a heat not much exceeding 300°.

The sawdust appeared to be reduced to a granulated coal, partly pulverulent, and partly clotted; the whole weighed 210 grains.

105 grains of this coal were put into a platina crucible, and were exposed to a red heat under a muffle. At the same

time, an equal quantity of charcoal made from the same oak sawdust, was placed in another vessel by the side of the former.

The charcoal was speedily consumed, and left some brownish-white ashes, which as usual, afforded alkali, with a trace of a sulphate, which was probably sulphate of potash.

On the contrary, the coal formed by the humid way, burned without flame, similar to the Kilkenny coal, and others which do not contain bitumen. It was very slowly consumed, like the mineral coals above mentioned, and left some pale red ashes, which weighed 2 grains. These ashes *did not yield* the smallest vestige of *alkali*, and the only saline substance which could be obtained, was a very small portion of sulphate of potash, which did not amount to more than $\frac{1}{5}$ of a grain; and it is probable, that had the coal been more copiously washed, even this small portion of the neutral salt would not have been obtained.

2.

At the time when the preceding experiment was began, I also put 480 grains of the oak sawdust into another matrass, and having added four ounces of common muriatic acid, the whole was suffered to remain during the period which has been mentioned.

At the end of the four months, the remainder of the acid was for the greater part driven off by heat not exceeding 300°. The sawdust then had the appearance of a brownish-black mass, on which about a pint of boiling distilled water was poured; the whole was decanted into a filter, was repeatedly washed, and was afterwards dried without heat. The sawdust then appeared, as I have observed, brownish-

black, and was pulverulent. It burned with some flame, emitted still a slight vegetable odour, and was reduced to ashes much sooner than the coal formed by sulphuric acid, but not so speedily as the oak charcoal. The ashes had an ochraceous appearance, and were almost devoid of any saline substance, excepting a very slight trace of muriate of potash.

These two experiments therefore prove,

1st. That wood may by sulphuric acid be converted into a coal which in its properties is very different from charcoal, although prepared from the same sort of wood; and that the coal thus formed by the action of sulphuric acid, resembles by its mode of burning, and by not affording any alkali when reduced to ashes, those mineral coals which are devoid of bitumen.

2dly. That wood may also be converted into a sort of coal by muriatic acid, but in this case some of the vegetable characters remain, although, like the former, not any alkali can be obtained from the ashes.

§ VIII.

Four different solutions have been proposed respecting that difficult problem in the natural history of minerals, *the origin and formation of coal.*

The first is, that pit-coal is an earth or stone chiefly of the argillaceous genus, penetrated and impregnated with bitumen.

But Mr. KIRWAN very justly remarks, that the insufficiency of this solution is demonstrated by Kilkenny and other coals which are devoid of bitumen, and also that the quantity of earthy or stony matter in the most bituminous coals bears no proportion to the weight of them.*

* Geological Essays, p. 316.

The second and most prevailing opinion is, that mineral coal is of vegetable origin, that the vegetable bodies have, subsequent to their being buried under vast strata of earth, been mineralized by some unknown process, of which, sulphuric acid has probably been the principal agent, and that by means of this acid, the oils of the different species of wood have been converted into bitumen, and a coaly substance has been formed.

The third opinion is that of ARDUINO; who conceives coal to be entirely of marine formation, and to have originated from the fat and unctuous matter of the numerous tribes of animals that inhabit the ocean.

And the fourth is Mr. KIRWAN'S opinion, who considers coal and bitumen to have been derived from the primordial chaotic fluid.*

The limits of this Paper will not permit me to enter into the various arguments and facts which have been adduced in the support of these different opinions; but the second, or that which regards the vegetable substances as the principal origin of coal, seems by much the most probable, because it is corroborated by the greater number of geological facts, as well as by many experimental results. Most of the former have however been stated in different works, and I shall therefore only notice a few of the latter which have occurred in the course of my experiments.

The observations of Dr. CORREA DE SERRA on the wood of the submarine forest at Sutton, on the coast of Lincolnshire, together with many similar accounts which have been published in the Philosophical Transactions and other works, demonstrate in the most satisfactory manner, that whether

* Geological Essays, p. 327.

vegetables are totally or partially buried under the waves or under the earth, they are not merely by such means converted even into the most imperfect sort of coal.* Some process therefore independent of these circumstances must have taken place, in order that the vegetable substances, such as ligneous matter, resin, oil, &c. should become coal and bitumen.

In a former Paper I have endeavoured to shew, that these changes are progressive, and having noticed the perfect state of the submerged wood at Sutton and other places, I next described the qualities of the different kinds of Bovey coal, which exhibit a series of gradual changes from bodies which retain the vegetable structure and texture, although imperfectly carbonized, to others in which almost the complete characters of the common mineral or pit-coal are absolutely established.

From the alder leaves in the schistus from Iceland, I obtained extractive vegetable matter, and although this was not

* In my Paper, "*On the Change of some of the proximate Principles of Vegetables* "*into Bitumen*," I have quoted the remarks of BERGMAN, VON TROIL, and others, on the compressed state of the trunks of the trees which have been converted into surturbrand, Bovey coal, and similar substances. The same observation has been also made by Dr. CORREA DE SERRA respecting the timber of the submarine forest at Sutton; and this is the more remarkable, as the submerged vegetables at Sutton do not exhibit any appearance of carbonization.

Dr. CORREA says, "In general the trunks, branches, and roots of the decayed "trees, were *considerably flattened*; which is a phenomenon observed in the surtur-"brand or fossil wood of Iceland, and which SCHEUCHZER remarked also in the "fossil wood found in the neighbourhood of the lake of Thun, in Switzerland." Phil. Trans. 1799, p. 147.

afforded by the varieties of Bovey coal, yet these, as well as the alder leaves, and also a coal like that of Bovey, found in Sussex, at Newick Park, (an estate belonging to Sir ELIJAH IMPEY,) and also the surturbrand of Iceland, yielded some resin, which at Bovey is likewise found in distinct masses, intermixed with the strata of coal, and combined with asphaltum, in the proportion of about 41 parts of the latter with 55 of resin.*

Now, exclusive of the other vegetable characters which are so evident in many of the varieties of Bovey coal, of the Sussex coal, of surturbrand, &c. &c. the presence of resin must be regarded as a strong fact; for this substance has always been attributed to the organized bodies, particularly to those of the vegetable kingdom, and I do not know of any instance, previous to my own experiments, in which, resin had been discovered as constituting part of any of the different species and varieties of coal.

From the external vegetable characters possessed by the Bovey coal, the Sussex coal, the surturbrand, and many others, together with the resin, (allowed to be exclusively a vegetable substance, or at least one which only appertains to the organized natural bodies,) there cannot be any doubt, that such coals have been formed from wood and other substances belonging to the vegetable kingdom.

But some mineralogists attempt to draw a line of separation between the coals above mentioned and the others, which therefore they call the true mineral coals.

* Observations on the Change of some of the proximate Principles of Vegetables into Bitumen. Phil. Trans. 1804, p. 405.

This opinion may in some degree be refuted even from the specimens afforded by the Bovey coal-pits, where, as I have observed, a regular gradation may be seen from wood which is but very imperfectly carbonized, to the substance called stone coal, which in every respect appears to be most nearly if not absolutely similar to the common pit-coals.*

It may however be objected, that such a transition is peculiar to this and similar places, and that the pit-coal found in other situations, where nothing resembling the Bovey coal can be discovered, is in reality of a different nature.

But this objection I think may be answered by the results of those experiments on pit-coal, Cannel coal, and asphaltum, which I have related in the third section of this Paper; for when these were subjected to the action of nitric acid not too long continued, it was found, that the acid first dissolved the principal part of the carbonaceous matter, and if then the process was stopped, there remained a substance in a proportion corresponding to that of the bitumen either in the pit-coal, or principally forming the Cannel coal and asphaltum, which although not absolutely in the state of resin, was however in a state intermediate between it and the vegetable extractive matter.

Moreover I have stated, that under similar circumstances, a substance possessing in a great measure the same properties, may be obtained from the known vegetable resins by the action of nitric acid.

When therefore, these facts are added to that of the

* Phil. Trans. 1804, p. 398.

natural mixture of resin and asphaltum which is found with the Bovey coal, we to all appearance have almost positive proof that the pit-coals are of vegetable origin.

True it is indeed, that bitumen has never been formed by any artificial process hitherto devised, from the resins or other vegetable substances. I have myself attempted it in various ways without success, for although I occasionally obtained products which resembled it somewhat in odour when burned, and other properties, yet the effects of alcohol or water always proved these products not to be bitumen.

But synthesis of natural products, although required in strict chemical demonstration, is (as we have but too often occasion to know) seldom to be attained, especially when operations are performed on bodies whose component parts are liable to an infinite series of variations in their proportions, qualities, and mode of combination.

Considering therefore, that bitumen and resin afford by certain operations similar products, that resin and bitumen are found blended together by nature, and that this mixed substance accompanies a species of coal which in many parts still exhibits its vegetable origin, whilst in others it passes into pit-coal, we may with the greatest probability conclude, that bitumen is a modification of the resinous and oily parts of vegetables, produced by some process of nature, which has operated by slow and gradual means on immense masses, so that even if we were acquainted with the process, we should scarcely be able to imitate its effects, from the want of time, and deficiency in the bulk of materials.

But although bitumen cannot at present be artificially

formed from the resinous and other vegetable substances by any of the known chemical processes, yet there is every reason to believe, that the agent employed by nature in the formation of coal and bitumen has been either muriatic or sulphuric acid; and when it is considered, that common salt is never found in coal mines except when in the vicinity of salt springs, whilst on the contrary, pyrites, sulphate of iron, and alum, most commonly are present;* these facts, together with the sulphureous odour emitted by most of the mineral coals when burned, appear strongly to evince the agency of the latter. That this has been the case, seems also to be corroborated, by the great resemblance which (as has been previously stated) the coals formed artificially from many vegetable substances bear to the mineral coals, especially as the similarity is not confined to external characters, but extends to other properties.

By the action of sulphuric acid on vegetable bodies, a much greater portion of their carbon is converted into coal than when the same are subjected to the effects of fire.

Several examples respecting the resins, have been mentioned in the seventh section of this Paper, and the result of the experiment made upon oak perfectly accords with them.

Mr. PROUST, in the course of some comparative experiments on the proportions of charcoal afforded by different kinds of wood, obtained 20 *per cent.* from green oak, and 19 *per cent.* from heart of oak.†

* KIRWAN's Geological Essays, p. 324.

† *Journal de Physique*, 1799, Tome 48, p. 469.

But by sulphuric acid, from 480 grains of oak, I obtained 210 grains, or about 45 *per cent.* of coal, which burned not like the charcoal obtained from the same wood, but like many of the mineral coals; and this was also observed in the combustion of the greater part of the coals obtained by the humid way from resinous substances.

The experiment on oak also appears to refute another objection to the vegetable origin of pit-coal, namely, the total absence of the alkalis, which on the contrary are so constantly obtained from the ligneous parts of vegetables by combustion.* But I have shewn, that when these bodies are carbonized in the humid way either by muriatic or by sulphuric acid, not any alkali can be obtained from the ashes of coals so formed; and this seems also to be a farther proof, that the humid way has been employed in the operations of nature to convert the above mentioned substances into pit-coal; for supposing fire to have been the agent, it does not appear easy to conceive how the alkali could have been destroyed or separated.†

* KIRWAN's Geological Essays, p. 320.

† Some have attempted to account for the absence of alkali in the Bovey coal and common pit-coal, by supposing that the vegetable bodies (from which these have been formed) were previously deprived of alkali by simple lixiviation during their immersion in water. But in page 127 of this Paper, I have shewn that the submerged oak of Sutton, although deprived of its tannin, still retained its potash, which certainly would not have been the case if the latter like the former could have been separated from the wood by mere solution. When wood is reduced to ashes, the alkali becomes completely denuded by the destruction of the woody fibre, and consequently may be immediately taken up by water; but when wood is converted into coal in the humid way by means of an acid, then it seems to me that two effects

Every circumstance seems therefore to support the opinion of those who consider the pit-coals as having been formed in the humid way, principally from vegetable bodies, and most probably by the agency of sulphuric acid; and allowing that animal substances may also have contributed to the production of coal, yet this would not militate against the above mentioned opinion, as the effects produced upon them by that acid would in all the essential points be perfectly similar.*

An inquiry into the nature and formation of coal was my first object when I discovered the artificial tanning substance,

take place; for the intimate combination of the alkali with the woody fibre becomes in a great measure destroyed by the carbonization of the latter, whilst a simultaneous action arises in the affinity between the acid and the alkali; so that if coal has been formed by such means, the alkali must have been separated from the wood in the state of a dissolved neutral salt.

* From the nature of the experiments which have been related in this Paper, I have unavoidably been induced to notice concisely the different opinions on the formation of coal by the humid way; but I did not intend to have mentioned any of those which have been brought forward in favour of the immediate or indirect action of fire, as I only wished to express my sentiments respecting the most probable of the former opinions.

Since however this Paper was written and partly read before the Royal Society, I have been favoured by Sir JAMES HALL, with a copy of his Paper, intitled "*Account of a Series of Experiments shewing the Effects of Compression in modifying the Action of Heat*;" and I am fully of opinion that the scientific world has not for a long time received any communication of more importance, or in which more accuracy, ability, and perseverance have been displayed. The effects which Sir JAMES HALL has produced on carbonate of lime by heat acting under compression, certainly removes a great and at one time apparently insurmountable obstacle to the HUTTONIAN or PLUTONIAN theory, and if they do

and considering the importance of the latter, it will not appear surprising, that it should immediately have engaged the principal part of my attention.

In addition to the experiments which have been related in the three Papers upon this subject, I intended to have decomposed the different varieties, to have compared their gases and other products with those of the natural substance called Tannin, and especially to have endeavoured to discover more economical methods of obtaining the artificial product; for, exclusive of speculative science, this appears to be an object of consequence, not only respecting that useful and valuable branch of manufacture to which it immediately relates, but also as the means of preventing, or at least of diminishing, the premature destruction of timber in a country, where, on account of its population, as well as on account of its maritime

not solve the grand geological problem, they must even, in an insulated point of view, be allowed to have opened a new and unexplored field of research in chemistry as well as in geology.

In the 8th section of this valuable Paper, the author has given an account of some experiments made on leather, horn, and fir sawdust, from which he obtained coal which burned with flame, and which apparently resembled some of the mineral coals. In one case also, he obtained a substance, which in external characters appeared somewhat similar to the mixture of asphaltum and resin found at Bovey, to which I have given the name of Retin-asphaltum. These experiments Sir JAMES HALL intends to resume, and it is my earnest wish that he would do so; for although I am strongly inclined to believe that the mineral coals have generally, if not always, been formed by some humid process, yet it is impossible to foresee the results which may be obtained from animal and vegetable bodies subjected to the effects of heat modified by compression, as the principles of these bodies may be acted upon, and may be made to re-act on each other, under circumstances which, until now have not been imagined.

position, every economy in such an article should be most rigidly observed.

But for the present, I intend to relinquish this subject to such as may consider it worthy of attention; whilst, as I have already stated, I entertain very sanguine expectations, that eventually it will prove economically useful; and should any be inclined to pursue the inquiry, I would recommend particular attention to those processes which relate to the roasted vegetable substances, and to peat.

Almost any refuse vegetable matter, such as twigs, dead leaves, &c. will serve for the former; whilst the latter, as I have shewn, does not require to be roasted, and in many, especially the northern counties, peat is found in such abundance, that but a small proportional quantity is consumed in the only useful way to which it has hitherto been applied, namely, fuel.

Before I conclude this Paper I shall also observe, that the experiments which have been described, must be regarded only as a mere sketch of that which may be performed, whilst the facts which have been ascertained respecting the resins, balsams, gum resins, and gums, serve to prove, that much may be expected from regular chemical examinations of these bodies. But such investigations, in order that science may truly be promoted, should be strictly regular: that is, they should not be taken up in a desultory manner, but these substances should be comparatively and systematically examined with all the accuracy which can be employed in the present state of chemical knowledge; for as this knowledge concerning the composition

of organized bodies is confessedly very imperfect, I am persuaded, that like other of the sciences, chemistry will be less liable to error, when guided by comparative experiments, and comparative analyses.

VII. *The Application of a Method of Differences to the Species of Series whose Sums are obtained by Mr.* Landen, *by the Help of impossible Quantities. By Mr.* Benjamin Gompertz. *Communicated by the Rev.* Nevil Maskelyne, *D. D. Astronomer Royal, F. R. S.*

Read February 13, 1806.

HAVING some years back, when reading the learned Mr. LANDEN's fifth Memoir, discovered the manner of applying a method of differences, to the species of series, whose sums are there obtained by the help of impossible quantities, and having since extended that application, I now venture to offer it to the consideration of others.

The practice of this method, in most cases, appears to me extremely simple; and on that account, I am almost induced to imagine, that they have already been considered by mathematicians; indeed since the greatest part of this Paper was written, I met with EULER's *Institutiones Calculi integralis;* two simple series are in that work summed by multiplications similar to those employed in the investigation of the principal theorems contained in this Paper; but whether that learned mathematician has farther pursued the method, in that or in any other work, I have not as yet been able to ascertain.

I have purposely considered some of the series summed by Mr. LANDEN, to afford an opportunity of comparing both the results and methods; and because the series may have parti-

cular cases in which both Mr. LANDEN's means and my own fail: I have added towards the end a general scholium concerning the cause, circumstances, and consequences of such failure in my method.

The foundation of the theorems depends on the following well known lemmas.

No. I.

2 sine of vz . sine of tz, is equal to
cos. of $\overline{t-v}\,.\,z$ — cos. of $\overline{t+v}\,.\,z$.

No. II.

2 sine of vz . cos. of tz, is equal to
sine of $\overline{t+v}\,.\,z$ — sine of $\overline{t-v}\,.\,z$, or
sine of $\overline{t+v}\,.\,z$ + sine of $\overline{v-t}\,.\,z$.

No. III.

2 cos. of vz . cos. of tz, is equal to
cos. of $\overline{t-v}\,.\,z$ + cos. of $\overline{t+v}\,.\,z$.

Theorem I.

If there be an infinite series a . sine of pz, $+\,b$. sine of $\overline{p+q}\,.\,z$, $+\,c$. sine of $\overline{p+2q}\,.\,z$, $+\,d$. sine of $\overline{p+3q}\,.\,z$ &c. $= s$, and from the series a, b, c, d, e, f, &c. there be continually formed new series

$$\left\{\begin{array}{l} a,\ a',\ b',\ c',\ d',\ e',\ \text{\&c.} \\ a,\ a'',\ b'',\ c'',\ d'',\ e'',\ \text{\&c.} \\ a,\ a''',\ b''',\ c''',\ d''',\ e''',\ \text{\&c.} \\ \text{\&c. \&c. \&c. \&c. \&c. \&c. \&c.} \end{array}\right\}$$

Every new series, being formed from that immediately above, by taking the differences of the terms, exactly in

the same manner as in the common differential method, except that they here continually commence with the first term, a; and if p' be put $= p - \frac{1}{2}q$, $p'' = p' - \frac{1}{2}q$, $p''' = p'' - \frac{1}{2}q$, $p^{iv} = p''' - \frac{1}{2}q$. &c. $s \,.\, 2$ sine of $\frac{1}{2}qz = s'$, $-s' \,.\, 2$ sine of $\frac{1}{2}qz = s''$, $s'' .\, 2$ sine of $\frac{1}{2}qz = s'''$, $-s''' .\, 2$ sine of $\frac{1}{2}qz = s^{iv}$ &c. Then shall

$$a \,.\, \text{cos. of } p'z + a' .\, \text{cos. of } \overline{p' + q} \,.\, z + b' .\, \text{cos. of } \overline{p' + 2q} \,.\, z + c' \text{ cos. of } \overline{p' + 3q} \,.\, z + \text{ \&c.} = s',$$

$$a \,.\, \text{sine of } p''z + a'' .\, \text{sine of } \overline{p'' + q} \,.\, z + b'' .\, \text{sine of } \overline{p'' + 2q} .\, z \text{ \&c.} = s'',$$

$$a \,.\, \text{cos. of } p'''z + a''' .\, \text{cos. of } \overline{p''' + q} .\, z + b''' .\, \text{cos. of } \overline{p''' + 2q} .\, z \text{ \&c.} = s''',$$

$$a \,.\, \text{sine of } p^{iv}z + a^{iv} .\, \text{sine of } \overline{p^{iv} + q} \,.\, z + b^{iv} .\, \text{sine of } \overline{p^{iv} + 2q} .\, z \text{ \&c.} = s^{iv},$$

&c, + &c, + &c, , =, &c.

For, multiplying the series $a \,.\,$ sine of $pz + b \,.\,$ sine of $\overline{p + q} \,.\, z + c$ .sine of $\overline{p + 2q} \,.\, z$ &c. $= s$, by $2 \,.\,$ sine of $\frac{1}{2}qz$ by lemma No. I. we get, $a \,.\,$cos. of $\overline{p - \frac{1}{2}q} \,.\, z - a \,.\,$ cos. of $\overline{p + \frac{1}{2}q} \,.\, z + b \,.\,$ cos. of $\overline{p + \frac{1}{2}q} \,.\, z - b \,.\,$ cos. of $\overline{p + \frac{3}{2} \,.\, q}z + c \,.\,$ cos. of $\overline{p + \frac{3}{2} \,.\, q}z - c \,.\,$ cos. of $\overline{p + \frac{5}{2}q} \,.\, z$ &c. $= s \,.\, 2$ sine of $\frac{1}{2}qz$ $\therefore$ putting $b - a = a'$, $c - b = b'$, $d - c = d'$, &c. $p - \frac{1}{2}q = p'$, $s \,.\, 2$ sine of $\frac{1}{2}qz = s'$ we have, $a \,.\,$ cos. of $p'z + a' .\,$ cos. of $\overline{p' + q} \,.\, z + b' .\,$ cos. of $\overline{p' + 2q} \,.\, z + c' .\,$ cos. of $\overline{p' + 3q} \,.\, z$ &c. $= s'$, multiply this by 2 sine of $\frac{1}{2}qz$ by help of lemma No. II. and we have $-a$ sine of $\overline{p' - \frac{1}{2}q} \,.\, z + a$ sine of $\overline{p' + \frac{1}{2}q} .\, z - a'$ sine of $\overline{p' + \frac{1}{2}q} .\, z + a'$ sine of $\overline{p' + \frac{3}{2}q} .\, z - b'$ sine of $\overline{p' + \frac{3}{2}q} .\, z + b'$ sine of $\overline{p' + \frac{5}{2}q} .\, z - c'$ sine of $\overline{p' + \frac{5}{2}q} .\, z + c'$ sine of $\overline{p' + \frac{7}{2}q} .\, z$ &c. $= s' .\, 2$ sine of $\frac{1}{2}qz$, put $b' - a' = a''$, $c' - b' = b''$, $d' - c' = d''$ &c. $p' - \frac{1}{2}q = p''$ and $-s' .\, 2$ sine of $\frac{1}{2}qz = s''$, and we have a sine of $p''z + a''$ sine of $\overline{p'' + q} \,.\, z + b''$ sine of $\overline{p'' + 2q} \,.\, z$ &c. $= s''$, and because this is exactly similar to the original equation, (if we put a'', b'', c'', &c. for b, c, d, &c. in that, and

p'' and s'' for p and s,) it follows that if we put $b''-a''=a'''$, $c''-b''=b'''$, $d''-c''=c'''$, &c. $p''-\frac{1}{2}q=p'''$, s''. 2 sine of $\frac{1}{2}qz=s'''$, that we shall have, a cos. of $p'''z+a'''$ cos. of $\overline{p'''+q}.z+b'''$, cos. of $\overline{p'''+2q}.z$ &c.$=s'''$, which is exactly similar to the second equation; (if a''', b''', c''', &c. p''' and s''' be written for a', b', c', &c. p' and s' in that,) and therefore putting $b'''-a'''=a^{iv}$, $c'''-b'''=b^{iv}$, $d'''-c'''=c^{iv}$ &c. $p'''-\frac{1}{2}q=p^{iv}$, $-s'''$. 2 sine of $\frac{1}{2}qz$ $=s^{iv}$, we get a sine of $p^{iv}z+a^{iv}$ sine of $\overline{p^{iv}+q}.z+b^{iv}$. sine of $\overline{p^{iv}+2q}.z$, &c.$=s^{iv}$, again, similar to the first, by putting a^{iv}, b^{iv}, c^{iv}, &c. p^{iv}, s^{iv} in that equation for b, c, d, &c. p and s, and thus do we continually get equations in form similar to the first and second equations QED.

Cor. I. $s''=-s'$. 2 sine of $\frac{1}{2}qz=-s.\overline{2\text{ sine of }\frac{1}{2}qz}|^2$, $s'''=s''$. 2 sine of $\frac{1}{2}qz=-s'.\overline{2\text{ sine of }\frac{1}{2}qz}|^2=-s.\overline{2\text{ sine of }\frac{1}{2}qz}|^3$, $s^{iv}=-s'''$. 2 sine of $\frac{1}{2}qz=s'.\overline{2\text{ sine of }\frac{1}{2}qz}|^3=s.\overline{2\text{ sine of }\frac{1}{2}qz}|^4$, and in general put $s^{(\pi)}$ to represent the πth successive value of s, and we shall have $s^{(\pi)}=\pm s'.\overline{2\text{ sine of }\frac{1}{2}qz}|^{\pi-1}=\pm s.\overline{2\text{ sine of }\frac{1}{2}qz}|^{\pi}$, the upper sign to be taken when π being divided by 4 leaves 0 or 1, the under when it leaves 2 or 3. πth successive value of $p=p-\pi.\frac{1}{2}q$, note the values s', s'', s''', &c. I call successive sums of s, and $s=\pm\frac{s^{(\pi)}}{\overline{2\text{ sine of }\frac{1}{2}qz}|^{\pi}}=\pm\frac{s^{(\pi-1)}}{\overline{2\text{ sine of }\frac{1}{2}qz}|^{\pi-1}}$

Corollory II. If A, B, C, &c. A′, B′, C′, &c. A″, B″, C″, &c. &c. be put for the series of the 1st, 2d, 3d differences &c. of the series a, b, c, &c. taken according to the common method of differences, we shall have the series

a, a', b', c', &c. the same as the series		a, A, B, C, D, &c.
a, a'', b'', c'', &c.	- - -	a, a'', A$'$, B$'$, C$'$, &c.
a, a''', b''', c''', &c.	- - -	a, a''', b''', A$''$, B$''$, &c.
$a, a^{iv}, b^{iv}, c^{iv}$, &c.	- - -	$a, a^{iv}, b^{iv}, c^{iv}$, A$'''$, &c.
&c. &c. &c. &c. &c.		&c. &c. &c. &c. &c. &c.

This is evident by taking the differences by both methods, and comparing them.

Cor. III. Likewise if A, B, C, &c. A$'$, B$'$, C$'$, &c. A$''$, B$''$, C$''$, &c. &c. be put for the series of the 1st, 2d, 3d, &c. differences of the series a, a', b', c', &c. found by the common method of differences, then shall the series

a, a'', b'', c'', d'', &c.	=	a, A, B, C, &c.
$a, a''', b''', c''', d'''$, &c.	=	a, a''', A$'$, B$'$, &c.
$a, a^{iv}, b^{iv}, c^{iv}, d^{iv}$, &c.	=	a, a^{iv}, b^{iv}, A$''$, &c.
&c. &c. &c. &c. &c. &c.		&c. &c. &c. &c. &c.

These things being known, we shall now propose some examples of their use.

Example 1. Required the sum of the infinite series sine of $pz+$ sine of $\overline{p+q}.z+$ sine of $\overline{p+2q}z+$ sine of $\overline{p+3q}.z$ &c.

Here a, b, c, &c. $=1, 1, 1, 1, 1$, &c. a, a', b', &c. $=1, 0, 0, 0, 0$, &c. $\Big\}$ therefore s' or $s.2$ sine of $\frac{1}{2}qz=$ cos. of $p'z=$ cos. of $\overline{p-\frac{1}{2}q}z$ $\therefore$ the sum $s=\dfrac{\text{cos. of } \overline{p-\frac{1}{2}q}.z}{2 \text{ sine of } \frac{1}{2}qz}$.

Cor. I. If p and q were each $=1$, we should have, sine of $z+$ sine of $2z+$ sine of $3z$ &c. $=\dfrac{\text{cos. of } \frac{1}{2}z}{2 \text{ sine of } \frac{1}{2}z}=\frac{1}{2}$ cotangent of $\frac{1}{2}z$.

Cor. II. If p were $=\frac{1}{2}q$, we should have sine of $pz+$ sine of $3pz+$ sine of $5pz$ &c. $=\dfrac{\text{cos. of } \overline{p-p}.z}{2 \text{ sine of } pz}=\dfrac{1}{2 \text{ sine of } pz}=\frac{1}{2}$ cosecant of pz.

Example 2, Required the sum of the infinite series, cos. of nz + cos. of $\overline{n+q}z$ + cos. of $\overline{n+2q}\,.\,z$ &c.

Here writing n in the room of p' we have

$\left.\begin{array}{l} a,\ a',\ b',\ c',\ \&c. = 1,\ 1,\ 1,\ 1,\ \&c. \\ a,\ a'',\ b'',\ c'',\ \&c. = 1,\ 0,\ 0,\ 0,\ \&c. \end{array}\right\}$ therefore s'' or $-s'.2$ sine of $\frac{1}{2}qz =$ sine of $p''z =$ sine of $\overline{n-\frac{1}{2}q}.z$ $\therefore$ s' the sum $= -\frac{\text{sine of } \overline{n-\frac{1}{2}q}\,.\,z}{2 \text{ sine of } qz}$.

Cor. I. If $n = \frac{1}{2}q$, we shall have cos. of nz + cos. of $3nz$ + cos. of $5nz$ &c. $= -\frac{\text{sine of } \overline{n-n}.z}{2 \text{ sine of } \frac{1}{2}qz} = 0$.

Cor. II. If $n = q$, we shall have cos. of nz + cos. of $2nz$ + cos. of $3nz$ &c. $= -\frac{\text{sine of } \frac{1}{2}qz}{2 \text{ sine of } \frac{1}{2}qz} = -\frac{1}{2}$.

Example 3, Required the sum of the infinite series, sine of nz + 4 sine of $\overline{n+q}.z$ + 9 sine of $\overline{n+2q}.z$ + 16 sine of $\overline{n+3q}.z$ &c. Here $p = n$

and $\left.\begin{array}{l} a,\ b,\ c,\ d,\ \&c. = 1,\ 4,\ 9,\ 16,\ 25,\ \&c. \\ a,\ a',\ b',\ c',\ \&c. = 1,\ 3,\ 5,\ 7,\ 9,\ \&c. \\ a,\ a'',\ b'',\ c'',\ \&c. = 1,\ 2,\ 2,\ 2,\ 2,\ \&c. \\ a,\ a''',\ b''',\ c''',\ \&c. = 1,\ 1,\ 0,\ 0,\ 0,\ \&c. \end{array}\right\}$ therefore s''' or $-s.\overline{2 \text{ sine of } \frac{1}{2}qz}|^3 =$ cos. of $p'''z$ + cos. of $\overline{p'''+q}.z =$ cos. of $\overline{n-\frac{3}{2}q}.z$ + cos of $\overline{n-\frac{1}{2}q}\,.\,z$, and therefore s the sum $= -\frac{\text{cos. of } \overline{n-\frac{3}{2}q}\,.\,z + \text{cos. of } \overline{n-\frac{1}{2}q}\,.\,z}{\overline{2 \text{ sine of } \frac{1}{2}qz}|^3}$.

Cor. I. If $n = \frac{1}{2}q$, we have, sine of nz + 4 sine of $3nz$ + 9 sine of $5nz$ &c. $= -\frac{\text{cos. of } \overline{-2nz} + 1}{\overline{2 \text{ sine of } nz}|^3} = -\frac{\text{cos. of } \overline{2nz} + 1}{\overline{2 \text{ sine of } nz}|^3} = -\frac{\text{versed sine supplement of } 2nz}{\overline{2 \text{ sine of } nz}|^3}$.

Cor. II. If $n = q$, we shall have, sine of nz + 4 sine of $2nz$ + 9 sine of $3nz$ &c. $= -\frac{\text{cos. of } \overline{-\frac{1}{2}nz} + \text{cos. of } \frac{1}{2}nz}{\overline{2 \text{ sine of } \frac{1}{2}nz}|^3} = -\frac{2 \text{ cos. of } \frac{1}{2}nz}{\overline{2 \text{ sine of } \frac{1}{2}nz}|^3}$, because, cos. of $-nz =$ cos. of $+nz$.

Scholium 1. It is evident from *Cor.* II. and III. *Theorem* I. that if the coefficients of the sines (a, b, c, &c.) or of the cosines (a, a', b', c', &c.) be such that any order of differences taken according to the common method becomes $=0$, we shall then have the corresponding value, of the successive values of s, s', s'', &c. expressed in finite terms, and we shall consequently get the value of the series sought expressed in finite terms, and likewise all the intermediate values of s', s'', s''', &c. contained between s and the said corresponding successive value of s, expressed in finite terms; hence if the values of a, b, c, &c. or of a, a', b', c', &c. be respectively equal to $\frac{r}{t}g$, $\frac{r\cdot\overline{r+h}}{t\cdot\overline{t+h}}g$, $\frac{r\cdot\overline{r+h}\cdot\overline{r+2h}}{t\cdot\overline{t+h}\cdot\overline{t+2h}}$ &c. r, h, t, being all affirmative values, and $r-t$ a multiple of h, we may obtain the sum of the series.

In order to prove this, I shall put r_1, r_2, r_3, &c. to represent $r+h$, $r+2h$, $r+3h$, &c. t_1, t_2, t_3, &c. for $t+h$, $t+2h$, $t+3h$, &c. then will the increment of $\frac{r\,r_1\,r_2 \ldots r_\nu}{t_\varepsilon\, t_{\varepsilon+1}\, t_{\varepsilon+2} \ldots t_{\varepsilon+\nu}} = \frac{r\,r_1 \ldots r_{\nu+1}}{t_\varepsilon \ldots t_{\varepsilon+\nu+1}} - \frac{r\,r_1 \ldots r_\nu}{t_\varepsilon \ldots t_{\varepsilon+\nu}}$ (ν being supposed the only variable quantity) $= \frac{r_{\nu+1}-t_{\varepsilon+\nu+1}}{t_\varepsilon} \times \frac{r\cdot r_1\cdot r_2 \ldots r_{\nu+1}}{t_{\varepsilon+1} \ldots t_{\varepsilon+\nu+1}} = \frac{r-t-\varepsilon h}{t+\varepsilon h} \times \frac{r\,r_1 \ldots r_\nu}{t_{\varepsilon+1} \ldots t_{\varepsilon+\nu+1}}$; it is likewise evident that the $\overline{\nu+1}$th term of the series proposed may be expressed by $\frac{r\,r_1\,r_2\,r_3 \ldots r_\nu}{t\,t_1\,t_2\,t_3 \ldots t_\nu}\cdot g$, ($\nu$ being a whole positive number,) this term we will call T, therefore we have, from

what has been just shown, $\dot{T} = \frac{r-t}{t} \cdot g \cdot \frac{\overset{r \,....\, r}{\nu}}{\underset{1}{t} \,....\, \underset{\nu+1}{t}}$, $\ddot{T} = \frac{r-t}{t} \cdot \frac{r-t-b}{t+b} \cdot g \times \frac{\overset{r \,....\, r}{\nu}}{\underset{2}{t} \,....\, \underset{\nu+2}{t}}$, $\dddot{T} = \frac{r-t}{t} \cdot \frac{r-t-b}{t+b} \cdot \frac{r-t-2b}{t+2b} \cdot g \times \frac{\overset{r \,.....\, r}{\nu}}{\underset{3}{t} \,.....\, \underset{\nu+3}{t}}$, &c. and the εth increment or difference $= \frac{r-t}{t} \cdot \frac{r-t-b}{t+b} \cdot \frac{r-t-2b}{t+2b}$ &c. $\ldots\ldots \times \frac{r-t-\overline{\varepsilon-1} \cdot b}{t+\overline{\varepsilon-1} \cdot b} g \times \frac{\overset{r \,.....\, r}{\nu}}{\underset{\varepsilon}{t} \,.....\, \underset{\nu+\varepsilon}{t}}$ which, it is evident, will be equal to o. If $r-t=\overline{\varepsilon-1} \,.\, h$ whatever ν may be, that is, whatever term of the εth order of difference be sought it will be found equal to o; the truth of this will be likewise evinced in particular cases by the following examples.

Example 4, Required the sum of the infinite series, $\frac{3}{1}$ sine of $pz + \frac{3.4}{1.2}$ sine of $\overline{p+q} \,.\, z + \frac{3.4.5}{1.2.3}$ sine of $\overline{p+q} \,.\, z$ &c.

$$\left.\begin{array}{l} \text{Here } a,\ b,\ c,\ d,\ \text{\&c.} = \frac{3}{1}, \frac{3.4}{1.2}, \frac{3.4.5}{1.2.3}, \frac{3.4.5.6}{1.2.3.4}, \text{ \&c.} \\ \quad a,\ a',\ b',\ c',\ \text{\&c.} = 3, \frac{2.3}{1.2}, \frac{2.3.4}{1.2.3}, \frac{2.3.4.5}{1.2.3.4}, \text{ \&c.} \\ \quad a,\ a'',\ b'',\ c'',\ \text{\&c.} = 3,\ \ 0,\ \ 1,\ \ 1,\ \ \text{\&c.} \\ \quad a,\ a''',\ b''',\ c''',\ \text{\&c.} = 3, -3,\ \ 1,\ \ 0,\ \ \text{\&c.} \end{array}\right\}$$

therefore s''' or $- s \,.\, \overline{2 \text{ sine of } \frac{1}{2}qz}|^3 = 3$ cos. of $p'''z - 3$ cos. of $\overline{p'''+q} \,.\, z +$ cos. of $\overline{p'''+2qz}$ $\therefore s$ the sum $= [3$ cos. of $p'''z - 3$ cos. of $\overline{p'''+q} \,.\, z +$ cos. $\overline{p'''+2q} \,.\, z] \div - \overline{2 \text{ sine of } \frac{1}{2}q \,.\, z}|^3 =$
$$\frac{3 \text{ cos. of } \overline{p-\frac{3}{2}q}\, z - 3 \text{ cos of } \overline{p-\frac{1}{2}q} \,.\, z + \text{cos of } \overline{p+\frac{1}{2}q} \,.\, z}{-\overline{2 \text{ sine of } \frac{1}{2}qz}|^3}.$$

Note. The series might have been written thus, 3 sine of $pz + 6$ sine of $\overline{p+q} \,.\, z + 10$ sine of $\overline{p+2qz}$. &c.

Example 5, Required the sum of the infinite series, $\frac{5}{3}$ cos. of $nz + \frac{5.6}{3.4}$ cos. of $\overline{n+q} \,.\, z + \frac{5.6.7}{3.4.5}$ cos. of $\overline{n+2qz}$ &c.

Here $p' = n$, therefore,

$$a,\ a',\ b',\ c',\ \&c. = \tfrac{5}{3},\ \frac{5.6}{3.4},\ \frac{5.6.7}{3.4.5},\ \frac{5.6.7.8}{3.4.5.6},\ \frac{5.6.7.8.9}{3.4.5.6.7},\ \&c.$$

$$a,\ a'',\ b'',\ c'',\ \&c. = \tfrac{5}{3},\ \frac{2.5}{3.4},\ \frac{2.5.6}{3.4.5},\ \frac{2.5.6.7}{3.4.5.6},\ \frac{2.5.6.7.8}{3.4.5.6.7},\ \&c.$$

$$a,\ a''',\ b''',\ c''',\ \&c. = \tfrac{5}{3},\ \frac{-2.5}{3.4},\ \frac{2.5}{3.4.5},\ \frac{2.5.6}{3.4.5.6},\ \frac{2.5.6.7}{3.4.5.6.7},\ \&c.$$

$$a,\ a^{iv},\ b^{iv},\ c^{iv},\ \&c. = \tfrac{5}{3},\ \frac{-5}{2},\ 1,\ 0,\ 0,\ \&c.$$

therefore s^{iv} or $s'.\overline{2 \text{ sine of } \tfrac{1}{2}qz}|^3 = \tfrac{5}{3}$ sine of $p^{iv}z - \tfrac{5}{2}$ sine of $\overline{p^{iv}+q}\,.z +$ sine of $\overline{p^{iv}+2q}\,.z$, and therefore s' the sum sought $= \dfrac{\tfrac{5}{3}\text{ sine of } p^{iv}z - \tfrac{5}{2}\text{ sine of } \overline{p^{iv}+q}.z + \text{ sine of } \overline{p^{iv}+2q}.z}{\overline{2 \text{ sine of } \tfrac{1}{2}qz}|^3} = [\tfrac{5}{3}$ sine of $\overline{n-\tfrac{3}{2}q}\,.z$ $-\tfrac{5}{2}$ sine of $\overline{n-\tfrac{1}{2}q}.z +$ sine of $\overline{n+\tfrac{1}{2}q}.z] : \overline{2 \text{ sine of } \tfrac{1}{2}qz}|^3$.

Note. The series might have been written thus, $\frac{4.5}{1.2}$ cos. of $nz + \frac{5.6}{1.2}$ cos. of $\overline{n+q}\,.z + \frac{6.7}{1.2}$ cos. of $\overline{n+2q}z$ &c.

Cor. If $2n = q$, s' becomes $\dfrac{\tfrac{5}{3}\text{ sine of } \overline{-2nz} - \tfrac{5}{2}\text{ sine of } 0 + \text{ sine of } 2nz}{\overline{2.\text{sine of } nz}|^3} = -\dfrac{\tfrac{2}{3}\text{ sine of } 2nz}{\overline{2 \text{ sine of } nz}|^3} = -\tfrac{4}{3}\dfrac{\text{sine of } nz \,..\, \text{cos. of } nz}{\overline{2 \text{ sine of } nz}|^3} = -\tfrac{1}{6}\dfrac{\text{cos. of } nz}{\overline{\text{sine of } nz}|^2}$, for the sum of the series, $\tfrac{5}{3}$ cos. of $nz + \frac{5.6}{3.4}$ cos. of $3nz + \frac{5.6.7}{3.4.5}$ cos. of $5nz$ &c. or its equal, $\frac{4.5}{12}$ cos. of $nz + \frac{5.6}{12}$ cos. of $3nz + \frac{6.7}{12}$ cos. of $5nz$ &c. $\therefore \dfrac{-2 \text{ cos. of } nz}{\overline{\text{sine of } nz}|^2} = 4.5$ cos. of $nz + 5.6.$ cos. of $3nz$ &c.

Scholium II. It is not always necessary for the differences of the coefficients to become equal to 0 to obtain the sum of the series, as will appear by

Example 6, Required the sum of the infinite series sine of $pz + g$ sine of $\overline{p+q}.z + g^2$ sine of $\overline{p+2q}.z + g^3$ sine of $\overline{p+3q}.z$ &c.

We have therefore

$a, b, c, d, e,$ &c. $= 1, g, \quad g^2, \quad g^3, \quad g^4,$ &c.

$a, a', b', c'\ d',$ &c. $= 1, g-1, g\times\overline{g-1}, g^2\times\overline{g-1}, g^3\times\overline{g-1},$ &c.

$a, a'', b'', c'', d'',$ &c. $= 1, g-2, \overline{g-1}|^2, g\,\overline{g-1}|^2, g^2.\overline{g-1}|^2,$ &c..

Consequently, s'' or $-s\,.\,\overline{2 \text{ sine of } \frac{1}{2}qz}|^2 =$ sine of $p''z+\overline{g-2}\,\cdot$ sine of $\overline{p''+q}.z+\overline{g-1}|^2.$ sine of $\overline{p''+2q}.z+\overline{g-1}|^2.g$ sine of $\overline{p''+3q}.z$ &c. $=$ sine of $\overline{p-q}.z+\overline{g-2}.$ sine of $pz+\overline{g-1}|^2.$ sine of $\overline{p+q}.z+\overline{g-1}|^2.g$ sine of $\overline{p+2q}.z+\overline{g-1}|^2.g^2$ sine of $\overline{p+3q}.z$ &c. but, $s =$ sine of $pz+g$ sine of $\overline{p+q}\,.z+g^2.$ sine of $\overline{p+2q}\,.z$ &c. Consequently, by multiplication, division, and transposition, $\overline{g-1}|^2$ sine of $\overline{p+q}.z+\overline{g-1}|^2\cdot g$ sine of $\overline{p+2q}.z+\overline{g-1}|^2\,g^2$ sine of $\overline{p+3q}.z$ &c. $= s.\frac{\overline{g-1}|^2}{g} - \frac{\overline{g-1}|^2}{g}.$ sine of pz, consequently the above equation becomes by substitution s'' or $-s.2.\text{sine of } \frac{1}{2}qz|^2 =$ sine of $\overline{p-q}.z+\overline{g-2}$ sine of $pz+\frac{\overline{g-1}|^2}{g}.s-\frac{\overline{g-1}|^2}{g}.$ sine of pz, therefore, s the sum required $=$

$$\frac{\text{sine of } \overline{p-q}.z+\overline{\overline{g-2}-\frac{\overline{g-1}|^2}{g}}.\text{ sine of } pz}{-\frac{\overline{g-1}|^2}{g}-2\text{ sine of }\frac{1}{2}qz|^2} = \frac{-g\text{ sine of }\overline{p-q}.z+\text{sine of } pz}{g^2+1-2g\text{ cos. of } qz},$$ and

by similar means, we have the sum of the series, cos. of pz $+g$ cos. of $\overline{p+q}\,.z+g^2$ cos. of $\overline{p+2q}\,.z$ &c. $=$ $\frac{-g\text{cos. of }\overline{p-q}.z+\text{ cos. of } pz}{g^2+1-2g.\text{ cos. of } qz}.$

Scholium III. Hitherto we have been considering, a series of sines and cosines, whose terms have all the same signs; but if the terms of a series proposed were alternately positive and negative, it would be necessary to divide them into two series, the one of the positive term and the other of the nega-

tive ; in order to get the sum by *Theorem* I. But the sum of a series whose terms are alternately positive and negative, may be obtained from the sum of a similar series, whose terms are all positive by a mere substitution; thus if the sum of the series, a sine of $rz - b$ sine of $\overline{r+s}\,.\,z + c$ sine of $\overline{r+2s}\,.\,z$—&c. were required, put $rz=180°-pz$, and $sz=180$ $-qz$, therefore the sine of $rz=$ sine of $\overline{180°-pz}=$sine of pz, sine of $\overline{r+s}.z=$ sine of $\overline{360°-\overline{p+q}.z}= -$ sine of $\overline{p+q}.z$, sine of $\overline{r+2s}\,.\,z=$sine $\overline{540°-\overline{p+2q}\,.\,z}=$sine of $\overline{p+2q}\,.\,z$, &c.; and consequently the sum of the series, a . sine of $rz - b$. sine of $\overline{r+s}.z+c$. sine of $\overline{r+2s}.z-$ &c.= the sum of the series, a sine of $pz+b$ sine of $\overline{p+q}\,.\,z+c$ sine of $\overline{p+2q}\,.\,z$ &c.; and by the like substitution may the sum of a series of cosines, whose terms are alternately positive and negative, be deduced from the sum of a series of cosines, whose terms are all positive: all this requires the functional values of p and q to be distinct, otherwise the substitution cannot be effected; but the said sum may be deduced at once by the following

Theorem II.

If there be a series, a . sine of $pz-b$. sine of $\overline{p+q}.z+c$. sine of $\overline{p+2q}\,.\,z-d$. sine of $\overline{p+3q}\,.\,z$ &c.$=s$, then shall

a . sine of $p'z-a'$. sine of $\overline{p'+q}.z+b'$. sine of $\overline{p'+2q}.z$ &c.:

a sine of $p''z-a''$ sine of $\overline{p''+q}.z+b''$ sine of $\overline{p''+2q}.z$ &c.:

a sine of $p'''z-a'''$ sine of $\overline{p'''+q}.z+b'''$ sine of $\overline{p'''+2q}.z$ &c.$=s'''$

&c. &c. &c. &c.

And if the series be,

a cos. of $pz-b$ cos. of $\overline{p+q}\,.z+c$ cos. of $\overline{p+2q}\,.z$ &c.$=s$,

then shall,

a cos. of $p'z-a'$ cos. of $\overline{p'+q}\,.z+b'$ cos. of $\overline{p'+2q}\,.z$ &c.$=s'$

a cos. of $p''z-a''$ cos. of $\overline{p''+q}.z+b''$ cos. of $\overline{p''+2q}.z$ &c.$=s''$

&c. &c. &c. &c.

a', a'', a''', &c. b', b'', b''', &c. c', c'', c''', &c. &c. being formed from a, b, c, d, e, &c. as in *Theorem* I. p', p'', p''', &c. likewise as in *Theorem* I. $s'=2s$. cos. of . $\frac{1}{2}qz$, $s''=2s'$. cos. of . $\frac{1}{2}qz$, $s'''=2s''$. cos. of $\frac{1}{2}qz$, &c.

First, if a sine of $pz-b$ sine of $\overline{p+q}\,.z+c$ sine of $\overline{p+q}\,.z$ &c. $=s$, by multiplying by 2 cos. of $\frac{1}{2}qz$, by lemma No. II. we shall have a. sine of $\overline{p-\frac{1}{2}q}\,.z+a$. sine of $\overline{p+\frac{1}{2}q}\,.z-b$. sine of $\overline{p+\frac{1}{2}q}.z-b$. sine of $\overline{p+\frac{3}{2}q}\,.z+c$. sine of $\overline{p+\frac{3}{2}q}\,.z$ &c.$=s$. 2 cos. of $\frac{1}{2}qz$; consequently, putting $b-a=a'$, $c-b=b'$, &c. $p-\frac{1}{2}q=p'$, $s'=2s$ cos. of $\frac{1}{2}qz$, we have, a sine of $p'z-a'$ sine of $\overline{p'+q}\,.z+b'$ sine of $\overline{p'+2q}\,.z$ &c.$=s'$, which being exactly similar in form to the original series, the other series will be deduced from this by continually proceeding in the same method.

Again, if a. cos. of $pz-b$. cos. of $\overline{p+q}\,.z+c$. cos. of $\overline{p+2q}\,.z$ &c.$=s$, we have by multiplying by 2 cos. of $\frac{1}{2}qz$ by the help of lemma No. III., a cos. of $p'z-a'$ cos. of $\overline{p'+q}.z+b'$ cos. of $\overline{p'+2q}\,.z$ &c.$=s$, which being exactly similar in form, to the original, we may obtain the other series, which are likewise similar in form by the same mode of proceeding.

Cor. the πth successive value of $s=s.\overline{2\text{ cos. of }\tfrac{1}{2}qz}|^{\pi}$, the πth successive value of $p=p-\pi\,.\frac{1}{2}$ and $s=\dfrac{\pi\text{th successive value of }s}{\overline{2\text{ cos. of }\tfrac{1}{2}qz}|^{\pi}}$.

Example 1. Required the sum of the series, sine of $pz-$

sine of $\overline{p+q}.z+$ sine of $\overline{p+2qz}-$ &c. and likewise of, cos. of $pz-$ cos. of $\overline{p+q}\,.z+$ cos. of $\overline{p+2q}\,.z-$ &c.

Here in both, $a,\ b,\ c,\ d,$ &c.$=1,\ 1,\ 1,\ 1,$ &c. therefore, in $a,\ a',\ b',\ c',$ &c.$=1,\ 0,\ 0,\ 0,$ &c. the first series we have, s' or $s\,.\,2$ cos. of $\frac{1}{2}qz=$ sine of $p'z$ and $\therefore s=\frac{\text{sine of } p'z}{2\text{ cos. of }\frac{1}{2}qz}=\frac{\text{sine of }\overline{p-\frac{1}{2}q}z}{2\text{ cos. of }\frac{1}{2}qz}$, and for the second series we have, s', or, $s\,.\,2$ cos. of $\frac{1}{2}qz=$ cos. of $\overline{p-\frac{1}{2}q}\,.\,z$, and therefore, $s=\frac{\text{cos. of }\overline{p-\frac{1}{2}q}.z}{2\text{ cos. of }\frac{1}{2}qz}$.

Cor. 1. If $p=q$ the first series will be, sine of $pz-$ sine of $2pz+$ sine of $3pz$ &c.$=\frac{\text{sine of }\frac{1}{2}pz}{2\text{ cos. of }\frac{1}{2}pz}=\frac{1}{2}$ tangent of $\frac{1}{2}pz$, and the second, cos. of $pz-$ cos. of $2pz+$ cos. of $3pz$ &c. $=\frac{\text{cos. of }-\frac{1}{2}pz}{2\text{ cos. of }\frac{1}{2}pz}=\frac{1}{2}$.

Scholium. Though we have given two theorems, the one for a series whose terms are all positive, and the other for a series whose terms are alternately positive and negative; they are both true whatever the signs of the terms be, provided that proper signs be used in the operation; that is, if any term should have a contrary sign, to the sign of that term contained in the enunciation of the theorem used, then a contrary sign must likewise be prefixed to it in the operation; thus, for instance, if for a series whose terms are all positive we should use *Theorem* II. or for a series whose terms are alternately positive and negative we should use *Theorem* I., for $a, b, c, d,$ &c. we must write $a, -b, c, -d,$ &c. and therefore

$a,\ a',\ b',$ &c.$=a, -\overline{a+b},\ \overline{c+b},\ -\overline{d+c},$ &c.$=$(suppose) $a, -a_{,}, b_{,}, -c_{,},$ &c.

$a,\ a'',\ b'',$ &c.$=a, -\overline{a_{,}+a},\ \overline{b_{,}+a_{,}},\ -\overline{c_{,}+b_{,}},$ &c.$=$ - - - - $a, -a_{,,}, b_{,,}, -c_{,,},$ &c.

$a,\ a''',\ b''',$ &c.$=a, -\overline{a_{,,}+a},\ \overline{b_{,,}+a_{,,}},\ -\overline{c_{,,}+b_{,,}},$ &c.$=$ - - - - - - &c.

&c. &c. &c. &c. &c. &c. &c. &c. &c. &c.

and consequently we shall get new series, the coefficients of whose terms are formed from the coefficients in the preceding series, by addition instead of subtraction; and may be of good purpose on some occasions. And if we alternately use these theorems the operation will be performed by alternately taking sums and differences; and this will amount to the same as taking the differences of the alternate terms, beginning always with two noughts: but, for the more readily comprehending this, we shall offer a theorem which moreover is the first of these theorems I discovered, but previously thereto shall propose

Example 2. Let the series be either of these, sine of $pz + r$ sine of $\overline{p+q}.z + r.\frac{r+1}{2}$ sine of $\overline{p+2q}.z + r.\frac{r+1}{2}.\frac{r+2}{3}$ sine of $\overline{p+3q}.z +$ &c., cos. of $pz + r$ cos. of $\overline{p+q}.z + r.\frac{r+1}{2}$. cos. of $\overline{p+2q}.z +$ &c., sine of $pz - r$ sine of $\overline{p+q}.z + r.\frac{r+1}{2}$ sine of $\overline{p+2q}.z -$ &c. or, cos. of $pz - r$ cos. of $\overline{p+q}.z + r.\frac{r+1}{2}$ cos. of $\overline{p+2q}.z -$ &c. r being a whole positive number, the terms in the two first series all positive, and in the two last alternately positive and negative.

The coefficients being,

Of 1st term. 2d. 3d. 4th. 5th.

$$1,\quad r,\quad r.\frac{r+1}{2},\quad \frac{r+1}{2}\ \frac{r+2}{3}\quad r.\frac{r+1}{2}.\frac{r+2}{3},\frac{r+3}{4}, \text{ \&c.}$$

the first differences

$$1, \frac{r-1}{1}, \frac{r-1}{1}.\frac{r}{2}, \frac{r-1}{1}.\frac{r}{2}.\frac{r+1}{3}, \frac{r-1}{1}.\frac{r}{2}.\frac{r+1}{3}.\frac{r+2}{4}, \text{ \&c.}$$

2d differences

$$1, \frac{r-2}{1}, \frac{r-2}{1}.\frac{r-1}{2}, \frac{r-2}{1}.\frac{r-1}{2}.\frac{r}{3}, \frac{r-2}{1}.\frac{r-1}{2}.\frac{r}{3}.\frac{r+1}{4}, \text{ \&c.}$$

3d differences

Of 1st term. 2d. 3d. 4th. 5th.

$$1, \frac{r-3}{1}, \frac{r-3}{1}\cdot\frac{r-2}{2}, \frac{r-3}{1}\cdot\frac{r-2}{2}\cdot\frac{r-1}{3}, \frac{r-3}{1}\cdot\frac{r-2}{2}\cdot\frac{r-1}{3}\cdot\frac{r}{4}, \text{\&c.}$$

And in general, πth differences

$1, \frac{r-\pi}{1}, \frac{r-\pi}{1}\cdot\frac{r-\pi-1}{2}, \frac{r-\pi}{1}\cdot\frac{r-\pi-1}{1}\cdot\frac{r-\pi-2}{3}$, &c. to be continued to $\overline{\pi+1}$ terms, and the remaining terms will be the $\overline{\pi+1}$th term multiplied by $\frac{r}{\pi+1}, \frac{r}{\pi+1}\cdot\frac{r+1}{\pi+2}, \frac{r}{\pi+1}\cdot\frac{r+1}{\pi+2}\cdot\frac{r+2}{\pi+3}$, &c. and consequently if π be $= r$, all the terms of the πth differences except the first will vanish. Hence we have by *Theorem* I. and its *Cor.* 1. the sum of the series, sine of $pz + r$ sine of $\overline{p+q}.z + r.\frac{r+1}{2}$. sine of $\overline{p+2q}.z+$ &c. $= \pm \frac{\text{sine of } \overline{p-\frac{1}{2}rq}.z}{\overline{2 \text{ sine of } \frac{1}{2}qz}|^r}$, if r be even, but $\pm \frac{\text{cos. of } \overline{p-\frac{1}{2}rq}.z}{\overline{2 \text{ sine of } \frac{1}{2}qz}|^r}$, if r be odd, the upper signs to be taken when r being divided by 4 leaves 0 or 1, and the under signs when it leaves 2 or 3. And the sum of the series, cos. of $pz + r$ cos. of $\overline{p+q}.z + r.\frac{r+1}{2}$ cos. of $\overline{p+2q}.z+$, &c. $= \pm \frac{\text{sine of } \overline{p-\frac{1}{2}rq}.z}{\overline{2 \text{ sine of } \frac{1}{2}qz}|^r}$ if r be odd, but $\pm \frac{\text{cos. of } \overline{p-\frac{1}{2}rq}.z}{\overline{2 \text{ sine of } \frac{1}{2}qz}|^r}$ if even, the upper signs to be taken, if r leaves 3 or 0 when divided by 4, and the under if it should leave 2 or 1. In deducing the sum of this series from the said *Cor.* it is necessary to put $\overline{p+\frac{1}{2}q}$ for p and $r+1$ for the π used there. The sum of the series, sine of $pz - r$ sine of $\overline{p+q}.z + r.\frac{r+1}{2}$ sine of $\overline{p+2q}$ $.z-$, &c. by *Theorem* II. is $= \frac{\text{sine of } \overline{p-\frac{1}{2}rq}.z}{\overline{2 \text{ cos. of } \frac{1}{2}qz}|^r}$: and the sum of the series, cos. of $pz - r$ cos. of $\overline{p+q}.z + r.\frac{r+1}{2}$ cos. of $\overline{p+2q}$ $.z -$ &c. by the same $= \frac{\text{cos. of } \overline{p-\frac{1}{2}rq}.z}{\overline{2 \text{ cos. of } \frac{1}{2}q}|^r}$.

Corollary. Because sine of $pz = pz - \frac{p^3 z^3}{2.3} + \frac{p^5 z^5}{2.3.4.5}$, &c. sine of $\overline{p+q}z = \overline{p+q}.z - \frac{\overline{p+q}|^3}{2.3}.z^3 + \frac{\overline{p+q}|^5}{2.3.4.5}z^5$, &c. sine of $\overline{p+2q}.z = \overline{p+2q}.z - \frac{\overline{p+2q}|^3}{2.3}.z^3 + \frac{\overline{p+2q}|^5}{2.3.4.5}.z^5$, &c. &c., and $\frac{1}{\overline{\text{cos. of } \frac{1}{2}qz}|^r} = \frac{1}{\overline{1 - \frac{\frac{1}{4}q^2 z^2}{1.2} + \frac{\frac{1}{16}q^4 z^4}{1.2.3.4}\ \&c.}|^r} = 1 + Az^2 + Bz^4 + Cz^6$, &c.

Where A, B, C, &c. stand for the coefficients of the multinomial, $1 - \frac{\frac{1}{4}q^2 z^2}{1.2} + \frac{\frac{1}{16}q^4 z^4}{1.2.3.4}$ &c. raised to the $-r$ power, and consequently r only concerned in them by pure powers; hence this being multiplied by $\overline{p-\frac{1}{2}qr}.z - \frac{\overline{p-\frac{1}{2}qr}|^3.z^3}{2.3} + \frac{\overline{p-\frac{1}{2}qr}|^5.z^5}{2.3.4.5}$ &c. the value of sine of $\overline{p-\frac{1}{2}qr}.z$, we obtain from the equation sine of $pz - r.$ sine of $\overline{p+q}.z + r.\frac{r+1}{2}$ sine of $\overline{p+2q}.z$, &c. $= \frac{\text{sine of } \overline{p-\frac{1}{2}qr}.z}{2\ \overline{\text{cos. of } \frac{1}{2}qz}|^r}$, the series $\overline{p - r.\overline{p+q} + r.\frac{r+1}{2}.\overline{p+2q} - \&c.}|.z - \overline{p^3 - r.\overline{p+q}|^3 + r.\frac{r+1}{2}\overline{p+2q}|^3 - \&c.}|.\frac{z^3}{2.3} + \overline{p^5 - r.\overline{p+q}|^5 + r.\frac{r+1}{2}.\overline{p+2q}|^5 - \&c.}|.\frac{z^5}{2.3.4.5}$, &c. $= \frac{p-\frac{1}{2}qr}{2^r}z - \overline{\frac{\overline{p-\frac{1}{2}qr}|^3}{2^r} - \frac{2.3A}{2^r}.\overline{p-\frac{1}{2}qr}}|.\frac{z^3}{2.3} + \overline{\frac{\overline{p-\frac{1}{2}q}|^5}{2^r} - \frac{4.5A}{2^r}.\overline{p-\frac{1}{2}qr}|^3 + \frac{B}{2^r}.\overline{p-\frac{1}{2}qr}}|.\frac{z^5}{2.3.4.5}$ &c., the law of continuation being evident in both series, consequently by comparing the homologous terms we obtain the sum of the series, $p - r.\overline{p+q} + r.\frac{r+1}{2}.\overline{p+2q} - r.\frac{r+1}{2}\frac{r+2}{3}.\overline{p+3q}$ &c. $= \frac{p-\frac{1}{2}qr}{2^r}$, of $p^3 - r.\overline{p+q}|^3 + r.\frac{r+1}{2}.\overline{p+2q}|^3$ &c. $= \frac{\overline{p-\frac{1}{2}qr}|^3}{2^r} - \frac{2.3}{2^r}.\overline{p-\frac{1}{2}qr}$, of the series $p^5 - r.\overline{p+q}|^5 + r.\frac{r+1}{2}.\overline{p+2q}|^5 -$ &c. $= \frac{\overline{p-\frac{1}{2}q}|^5}{2^r} - \frac{4.5.A}{2}.\overline{p-\frac{1}{2}qr}|^3 + \frac{2.3.4.5\,B}{2^r}.\overline{p-\frac{1}{2}qr}$, and so for the other odd powers, r being only concerned in these expressions

by pure powers, and by similar means we may from the equation, cos. of $pz - r.\text{cos. of } \overline{p+q}\,.\,z + r.\,\frac{r+1}{2}\,\text{cos. of } \overline{p+2q}\,.\,z$ &c. $= \frac{\text{cos. of } \overline{p-\frac{1}{2}qrz}}{2\,\overline{\text{cos. of } \frac{1}{2}qz}|^r}$, obtain the series $\overline{1 - r + r.\,\frac{r+1}{2} - r.\,\frac{r+1}{2}\,.\,\frac{r+2}{3}}$ &c. $- \overline{p^2 - r.\overline{p+q}|^2 + r\,.\,\frac{r+1}{2}.\overline{p+2q}|^2 - \&c.}\;\frac{z^2}{1.2} + \overline{p^4 - r.\overline{p+q}|^4 + r\,.\,\frac{r+1}{2}\,.\,\overline{p+2q}|^4 - \&c.}\Big|\,.\,\frac{z^4}{1.2.3.4} - \&c. = \frac{1}{2^r} - \frac{\overline{p-\frac{1}{2}qr}|^2 - 1.2.A}{2^r}\,.\,\frac{z^2}{1.2} + \frac{\overline{p-\frac{1}{2}qr}|^4 - 3.4.A.\overline{p-\frac{1}{2}qr}|^2 + 1.2.3.4.B}{2^r}\,.\,\frac{z^4}{1.2.3.4}$; and consequently by again comparing the homologous terms, we find $1 - r + r\,.\,\frac{r+1}{2} - r\,.\,\frac{r+1}{2}\,.\,\frac{r+2}{3}$ &c. $= \frac{1}{2^r}$, as it is well known to be, $p^2 - r\,.\,\overline{p+q}|^2 + r\,.\,\frac{r+1}{2}\,\overline{p+2q}|^2 -$ &c. $= \frac{\overline{p-\frac{1}{2}qr}|^2 - 1.2.A}{2^r}$, $p^4 - r.\overline{p+q}|^4 + r.\,\frac{r+1}{2}\,\overline{p+2q}|^4$ &c. $= \frac{\overline{p-\frac{1}{2}qr}|^4 - 3.4.A.\overline{p-\frac{1}{2}qr}|^2 + 1.2.3.4.B}{2^r}$, and so for the other even powers, r being only concerned in these expressions by pure powers.

Hence r being a whole positive number, the sum of the series, $p^m - r.\overline{p+q}|^m + r.\,\frac{r+1}{2}\,.\,\overline{p+2q}|^m - r\,.\,\frac{r+1}{2}\,.\,\frac{r+2}{3}.\overline{p+3q}|^m$ &c, m likewise being a whole positive number, may be always expressed by $\frac{1}{2^r} \times$ by a series of finite terms of pure powers of r whose coefficients are given, of the form $a + br + cr^2$ &c. p, q, and m being given values, and a, b, c, &c. determinate values independent of r; merely by comparing the coefficients of the homologous powers, of z, in the two equations of the series above. Now if we can prove that the same expressions, derived from the comparison of the coefficients of the homologous powers of z, give the sum of the series $p^m - r\,.\,\overline{p+q}|^m + r\,.\,\frac{r+1}{2}\,.\,\overline{p+2q}^m$ &c. whether r be a whole positive number

or not, it will follow that the series, sine of $pz - r$ sine of $\overline{p+q}.z + r.\frac{r+1}{2}$ sine of $\overline{p+2q}.z$ &c. will be equal to $\frac{\text{sine of } \overline{p-\frac{1}{2}qr.z}}{\overline{2\text{ cos. of }\frac{1}{2}qz}|^r}$ and the series, cos. of $pz - r$ cos. of $\overline{p+q}.z + r.\frac{r+1}{2}$ cos. of $\overline{p+2q}.z$ &c. $= \frac{\text{cos. of } \overline{p-\frac{1}{2}qr.z}}{\overline{2\text{ cos. of }\frac{1}{2}qz}|^r}$, whether r be a whole positive number or not.

And in order to prove this requisite, we shall first premise that if we have the sum of the series $p^m - r.\overline{p+q}|^m + r.\frac{r+1}{2}.\overline{p+2q}|^m - r.\frac{r+1}{2}.\frac{r+2}{3}.\overline{p+3q}|^m$ &c. whatever r may be, (m, p and q being given quantities) expressed by a series, $\frac{1}{2^r} \times \overline{A+Br+Cr^2}$ &c. of finite terms in which the *functional* values of p and r are distinct, A, B, C, &c. being given quantities independent of r, we may likewise find the sum of the series $p^{m+1} - r.\overline{p+q}|^{m+1} + r.\frac{r+1}{2}\overline{p+2q}|^{m+1}$ &c. for this series is equal to $p.p^m - r.\overline{p+q}.\overline{p+q}|^m + r.\frac{r+1}{2}.\overline{p+2q}.\overline{p+2q}|^m$ &c. $= p \times \overline{p^m - r.\overline{p+q}|^m + r.\frac{r+1}{2}.\overline{p+2q}|^m \text{ &c.}} - rq \times \overline{\overline{p+q}|^m - \overline{r+1}\,\overline{p+2q}|^m + \overline{r+1}\frac{r+2}{2}\overline{p+3q}|^m - \overline{r+1}.\frac{r+2}{2}.\frac{r+3}{3}.\overline{p+4q}|^m \text{ &c.}}$ but $p^m - r.\overline{p+q}|^m + r.\frac{r+1}{2}.\overline{p+2q}|^m$ &c. is equal to $\frac{1}{2^r} \times \overline{A+Br+Cr^2 \text{ &c.}}$ and if in this we write $\overline{r+1}$ for r and $\overline{p+q}$ for p, we shall have the sum of the series $\overline{p+q}|^m - \overline{r+1}.\overline{p+2q}|^m + \overline{r+1}\frac{r+2}{2}.\overline{p+3q}|^m$ &c. $= \frac{1}{2^{r+1}} \times \overline{A_{,} + B_{,}.\overline{r+1} + C_{,}.\overline{r+1}|^2 \text{ &c,}}$ $A_{,}$ $B_{,}$ $C_{,}$ &c. standing for the values that A, B, C, &c. become by writing $\overline{p+q}$ for p; and this may evidently be reduced to an expression of the finite terms of the form $\frac{1}{2^r} \times \overline{A' + B'r + C'r^2 \text{ &c.}}$ and con-

sequently will the sum of the series $p^{m+1}-r.\overline{p+q}|^{m+1}+r.\frac{r+1}{2}.\overline{p+2q}|^{m+1}$ &c. be $=$ the expression $\frac{1}{2^r}\times\overline{pA+pBr+pCr^2 \text{ \&c.} - qA'r - qB'r^2 \text{ \&c.}}$ of finite terms. This being proved, it follows, because the sum of the series $p^m-r.\overline{p+q}|^m+r.\frac{r+1}{2}.\overline{p+2q}|^m$ &c. when m is equal to 0, is equal to $1-r+r.\frac{r+1}{2}$ &c.$=$, by the binomial theorem, $\frac{1}{2^r}$, whatever r may be, that the sum of the series $p-r.\overline{p+q}+r.\frac{r+1}{2}\overline{p+2q}$ &c. namely, the said series when m is equal to 1, may be expressed by $\frac{1}{2^r}\times$ a series of pure powers of r of a finite number of terms whatever r may be, and comes out by the bye $\frac{1}{2^r}\times\overline{p-\frac{1}{2}qr}$, the same as above, and consequently by writing 1, 2, 3, 4, 5, 6, &c. one after the other for, m, we shall find that the sum of the series $p^m-r.\overline{p+q}|^m+r.\frac{r+1}{2}\overline{p+2q}|^m$ &c. may always be expressed by $\frac{1}{2^r}$ multiplied by a series of finite terms in the form $A+Br+Cr^2$ &c. A, B, C, &c. p, q, m, &c. being independent of r; and m a whole positive number. And these will, we shall prove without running through all the infinite cases, be the very same expressions as those given above, by comparing the coefficients of the homologous powers of z. In order to this we observe, since we have just proved that the sum of the said series, whatever r may be, may be expressed by $\frac{1}{2^r}\times$ series $\overline{A+Br+Cr^2}$ &c. of a finite number of terms, and from the comparison of the homologous powers, that when r is a whole number it may be expressed by $\frac{1}{2^r}\times$ value $a+br+cr^2$ &c. of a finite number of terms, it follows that when r is any

whole number, that these two values must then be equal to each other, and $\therefore$ that $A+Br^2+Cr^4$ &c. containing a finite number of terms must be then equal to $a+br+cr^2$ &c. containing a finite number of terms, and consequently the highest power of r and its coefficient must be the same in both series, otherwise by increasing r by the same in both, one side of the equation would become greater than the other, which is absurd; consequently the highest power of r and its coefficient is the same in both, and will destroy each other, and consequently the next highest powers of r and likewise their coefficients must be the same with each other, and will therefore be destroyed, &c. Hence the powers of r and their respective coefficients being the same in both, the expressions themselves must be the same in every respect, whether r be a whole number or not.

Hence we have not only given two different means of summing the series $p^m - r.\overline{p+q}|^m$ &c. (m being a whole positive number) whatever r may be, which indeed was not our chief object, but we have likewise proved that the series sine of $pz - r$ sine of $\overline{p+q}.z$ &c. $= \frac{\text{sine of } \overline{p-\frac{1}{2}qr.z}}{2 \cos \text{ of } \frac{1}{2}qz|^r}$, and the series cos. of $pz - r$ cos. of $\overline{p+q}.z$ &c. $= \frac{\text{cos. of } \overline{p-\frac{1}{2}qr.z}}{2 \text{ cos. of } \frac{1}{2}qz|^r}$, whatever r may be, the same as LANDEN finds.

Cor. II. Because these two series are equally true, whatever p may be, if for p we write $qr-p$ throughout, in the first we shall have, sine of $\overline{qr-p}.z - r$ sine of $\overline{\overline{r+1}.q-p}.z + r.\frac{r+1}{2}$ sine of $\overline{\overline{r+2}.q-p}.z$ &c. $= \frac{\text{sine of } \overline{\frac{1}{2}qr-p.z}}{2 \text{ cos. of } \frac{1}{2}qz|^r} = - \frac{\text{sine of } \overline{p-\frac{1}{2}q\,r\,z}}{2 \text{ cos. of } \frac{1}{2}q|^r}$.

Consequently sine of $\overline{qr-p}.z - r$ sine of $\overline{\overline{r+1}q-p}.z +$ &c.

$= -$ sine of $pz + r$ sine of $\overline{p+q}.z -$, &c. and from cos. of $pz - r$ cos. of $\overline{p+q}.z$, &c. $= \frac{\text{cos. of } \overline{p - \frac{1}{2}qr}.z}{2 \text{ cos. of } \frac{1}{2}qz|^r}$ by the like substitution we get cos. of $\overline{qr-p}.z - r$ cos. of $\overline{\overline{r+1}.q-p}.z + r.\frac{r+1}{2}$ cos. of $\overline{\overline{r+2}.q-p}.z$ &c. $= \left(\frac{\text{cos. of } \overline{\frac{1}{2}qr-p}\, z}{2 \text{ cos. of } \frac{1}{2}qz|^r} = \frac{\text{cos. of } \overline{p-\frac{1}{2}qr}.z}{2 \text{ cos. of } \frac{1}{2}qz|^r} = \right)$ cos. of $pz - r$ cos. of $\overline{p+q}.z + r.\frac{r+1}{2}$ cos. of $\overline{p+2q}.z$ &c. and these by *Cor.* I. are true, whatever r may be.

Cor. III. If p be $= \frac{1}{2}qr$ we shall have, cos. of $\frac{1}{2}qrz - r$ cos. of $\frac{1}{2}q\,\overline{r+2}.z + r.\frac{r+1}{2}$. cos. of $\frac{1}{2}q.\overline{r+4}.z$ &c. $= \frac{\text{cos. of } 0}{2 \text{ cos. of } \frac{1}{2}qz|^r} = \frac{1}{2 \text{ cos. of } \frac{1}{2}qz|^r}$, or if A be written for $\frac{1}{2}qz$ we shall have, cos. of $r\text{A} - r$ cos. of $\overline{r+2}\,\text{A} + r.\frac{r+1}{2}$ cos. of $\overline{r+4}.\text{A} -$ &c. $= \frac{1}{2 \text{ cos. of A}|^r}$ which is the same in substance as SIMPSON'S lemma, page 67 of his Tracts.

Cor. IV. If we put $pz = 180° - tz$, $qz = 180° - sz$ we shall have according to *Scholium* III. at the end of the examples to *Theorem* I.

sine of $tz + r$ sine of $\overline{t+s}.z + r.\frac{r+1}{2}$ sine of $\overline{t+2s}.z$ &c. $= \frac{\text{sine of } 180° - tz - \frac{1}{2}r.\overline{180° - sz}}{2 \text{ cos. of } 90° - \frac{1}{2}qz|^r} = \frac{\text{sine of } 90°\ \overline{r+t-\frac{1}{2}rs}.z}{2 \text{ sine of } \frac{1}{2}sz|^r}$ and $-$ cos. of $tz - r$ cos. of $\overline{t+s}.z - r.\frac{r+1}{2}$ cos. of $\overline{t+2s}.z$ &c. $= \frac{\text{cos. of } 180° - tz - \frac{1}{2}r.\overline{180° - sz}}{2 \text{ cos. of } 90° - \frac{1}{2}sz|^r} = -\frac{\text{cos. of } 90° + \overline{t - \frac{1}{2}rs}.z}{2 \text{ of sine } \frac{1}{2}sz|^r}$ $\therefore$ cos. of $tz + r$ cos. of $\overline{t+s}.z + r.\frac{r+1}{2}$ cos. of $\overline{t+2s}.z$ &c. $= \frac{\text{cos. of } 90°\ \overline{r+t-\frac{1}{2}rs}.z}{2 \text{ sine of } \frac{1}{2}sz|^r}$ and if in these r be a whole number, and p and q be written

for t and s, we shall have the results determined above. Many more corollaries may be derived from these.

Theorem III.

If there be formed a series of terms

$$\begin{array}{ccccccc} a, & b, & c, & d, & e, & f, & \&c. \\ a, & b, & a', & b', & c', & d', & \&c. \\ a, & b, & a'', & b'', & c'', & d'', & \&c. \\ a, & b, & a''', & b''', & c''', & d''', & \&c. \\ \&c, & \&c, & \&c, & \&c, & \&c, & \&c, & \&c. \end{array}$$

The terms of each series being formed from those immediately above, by taking the alternate differences of the terms, always beginning with o, o; that is, taking o from the 1st term, o from the 2d term, 1st term from 3d term, 2d term from 4th term, &c. in any of the series, for 1st, 2d, 3d, &c. terms of the next series. And p' be put $= p - q$, $p'' = p' - q$, $p''' = p'' - q$, &c. $s' = s$. 2 sine of qz, $s'' = -s'$. 2 sine of qz, $s''' = s''$. 2 sine of qz, $s^{\text{iv}} = -s'''$. 2 sine of qz, &c. I say if there be a series a sine of pz + b sine of $\overline{p+q}\,.\,z$ + c sine of $\overline{p+2q}\,.\,z$ + d sine of $\overline{p+3q}\,.\,z$, &c. $= s$, we shall have,

a cos. of $p'z$ + b cos. of $\overline{p'+q}\,.\,z$ + a' cos. of $\overline{p'+2q}\,.\,z$ + b' cos. of $\overline{p+3q}\,.\,z$, &c. $= s'$

a sine of $p''z$ + b sine of $\overline{p''+q}\,.\,z$ + a'' sine of $\overline{p''+2q}\,.\,z$ + b'' sine of $\overline{p+3q}\,.\,z$, &c. $= s''$, &c.

For multiplying the first of these by 2 sine of qz, by help of lemma No. I. we shall have, a . cos. of $\overline{p-q}\,.\,z$ — a . cos. of $\overline{p+q}\,.\,z$ + b . cos. of pz — b . cos. of $\overline{p+2q}\,.\,z$ + c cos. of $\overline{p+q}\,.\,z$ — c cos. of $\overline{p+3q}\,.\,z$ + d cos. of $\overline{p+2q}\,.\,z$ — d cos. of $\overline{p+4q}$

$.z + e$ cos. of $\overline{p+3q}\,.z$, &c. $= s\,.\,2$ sine of qz, therefore putting $a'=c-a$, $b'=d-b$, $c'=e-c$, &c. $p'=p-q$, $s'=s\,.\,2$ sine of qz, we get a cos. of $p'z+b$ cos. of $\overline{p'+q}\,.z + a'$ cos. of $\overline{p'+2q}$ $.z + b'$ cos. of $\overline{p+3q}\,.z$, &c. $= s'$, and multiplying this by 2 sine of qz, by help of lemma No. II. we get $a\,.$ sine of $\overline{p'+q}$ $z - a$ sine of $\overline{p'-q}\,.z + b$ sine of $\overline{p'+2q}\,.z - b$ sine of $p'z + a'$. sine of $\overline{p'+3q}\,.z - a'$ sine of $\overline{p'+q}\,.z + b'$ sine of $\overline{p'+4q}\,.z$ $- b'$. sine of $\overline{p'+2q}\,.z$ &c. $= s'\,.\,2$ sine of qz, therefore putting $a'-a=a''$, $b'-b=b''$, $c'-a'=c''$ &c. $p'-q=p''$, $-s'\,.$ 2 sine of $qz = s''$ we have, $a\,.$ sine of $p''z + b\,.$ sine of $\overline{p''+q}\,.z$ $+ a''$. sine of $\overline{p''+2q}\,.z + b''$. sine of $\overline{p''+3q}\,.z$ &c. $= s''$, which being exactly similar in form to the original series, the successive series, which will be of a similar form to the second or first of the series, will be deduced by the like operations and substitutions. Q. E. D.

Corollary I. The πth successive value of s is $= \pm s\,.\,\overline{2 \text{ sine of } qz}|^{\pi}$ or $\pm s'.\,\overline{2 \text{ sine of } qz}|^{\pi-1}$, the upper sign to be taken when π being divided by 4 leaves 0 or 1, otherwise the under sign and the πth successive value of $p = p - \pi\,.q$.

Corollary II. These operations are performed by differences whether the signs be all positive, or alternately positive and negative.

Example 1. Required the sum of the series n sine of $pz + \overline{n+r}$ sine of $\overline{p+q}\,.z + \overline{n+2r}$ sine of $\overline{p+2q}\,.z$ &c.

$$\text{Here } \left.\begin{array}{llllllll} a, & b, & c, & d, & \&c. = n, & n+r, & n+2r, & n+3r, \; n+4r, \; \&c. \\ a, & b, & a', & b', & \&c. = n, & n+r, & 2r, & 2r, \quad 2r, \quad \&c. \\ a, & b, & a'', & b'', & \&c. = n, & n+r, & 2r-n, & r-n, \quad 0, \quad \&c. \end{array}\right\}.$$

$\therefore s'' = -s\,.\,\overline{2 \text{ sine of } qz}|^{2} = n$ sine of $p''z + \overline{n+r}$ sine of $\overline{p''+q}$

$. z + \overline{2r-n}$ sine of $\overline{p''+2q} . z + \overline{r-n}$ sine of $\overline{p''+3q} . z$ ∴ by restoration and division we have s the sum $= [n$ sine of $\overline{p-2q} . z + \overline{n+r}$ sine of $\overline{p-q} . z + \overline{2r-n}$ sine of $pz + \overline{r-n}$ sine of $\overline{p+q} . z] \div -\overline{2 \text{ sine of } qz}|^2$: had we used *Theorem* I. we should have gotten a more simple valuation; namely, $s = \frac{n \text{ sine of } \overline{p-q} . z + \overline{r-n} \text{ sine of } pz}{-\overline{2 \text{ sine of } \frac{1}{2}qz}|^2}$ which is reducible to the other by multiplying the upper and under terms by $\overline{2 \text{ cos. of } \frac{1}{2}qz}|^2$ by help of lemma No. II. and III. Had the terms been alternate positive and negative we should have had

$$\left.\begin{array}{llllll} a, b, c, d, e, \&c. = n, & -\overline{n+r}, & \overline{n+2r}, & -\overline{n+3r}, & \overline{n+4r}, & \&c. \\ a, b, a', b', c', \&c. = n, & -\overline{n+r}, & +\ 2r, & -\ 2r, & +\ 2r, & \&c. \\ a, b, a'', b'', c'', \&c. = n, & -\overline{n+r}, & \overline{2r-n}, & -\ \overline{r-n}, & 0, & \&c. \end{array}\right\}$$

and therefore $s = [n$ sine of $\overline{p-2q} . z - \overline{n+r}$ sine of $\overline{p-q} . z + \overline{2r-n}$ sine of $pz - \overline{r-n}$ sine of $\overline{p+q} . z] \div -\overline{2 \text{ sine of } qz}|^2$. If we had used *Theorem* II. we should have obtained $s = \frac{n \text{ sine of } \overline{p-q} . z - \overline{r-n} . \text{ sine of } pz}{\overline{2 \text{ cos. of } \frac{1}{2}qz}|^2}$ which is reducible to the other by multiplying the upper and under terms by $\overline{2 \text{ sine of } \frac{1}{2}qz}|^2$, by help of lemma No. I. and II.

Theorem IV.

If there be a series, a . sine of $pz + b$. sine of $\overline{p+q} . z - c$. sine of $\overline{p+2q} . z - d$. sine of $\overline{p+3q} . z +$ &c. $= s$ or a cos. of $pz + b$ cos. of $\overline{p+q} . z - c$ cos. of $\overline{p+2q} . z - d$. cos. of $\overline{p+3q} . z +$ &c. $= s$ the signs of the terms changing alternately two by two; then in the first case

a sine of $p'z + b$ sine of $\overline{p' + q}.z - a'$ sine of $\overline{p' + 2q}.z - b'$ sine of $\overline{p' + 3q}.z +$ &c. $= s'$

a sine of $p''z + b$ sine of $\overline{p'' + q}.z - a''$ sine of $\overline{p'' + 2q}.z - b''$ sine of $\overline{p'' + 3q}.z +$ &c. $= s''$

&c. &c. &c. &c.

and in the second case

a cos. of $p'z + b$ cos. of $\overline{p' + q}.z - a'$ cos. of $\overline{p' + 2q}.z - b'$ cos. of $\overline{p' + 3q}.z +$ &c. $= s'$

a .cos. of $p''z + b$ cos. of $\overline{p'' + q}.z - a''$ cos. of $\overline{p'' + 2q}.z - b''$ cos. of $\overline{p'' + 3q}.z +$ &c. $= s''$

&c. &c. &c. &c.

where the terms a, b, a', b', c', &c. a, b, a'', b'', c'', &c. &c. are formed by taking the alternate differences, as in the last theorem; p', p'', p''', &c. likewise as in that theorem, $s' = s$. 2 cos. of qz, $s'' = \overline{2 \text{ cos. of } qz}|^2$, $s''' = \overline{2 \text{ cos. of } qz}|^3$ &c.

This is plain by multiplying the series continually by 2 cos. of qz by help of lemma No. II. for case 1, and lemma No. III. for case 2.

Example. Required the sum of the series, sine of $z +$ sine of $2z -$ sine of $3z -$ sine of $4z +$ &c.

Here $p = q = 1$ a, b, c, d, &c. $= 1, 1, 1, 1, 1$, &c. a, b, a', b', &c. $= 1, 1, 0, 0, 0$, &c. $\Big\} \therefore s' = s$. 2 cos. of z $=$ sine of $0z +$ sine of z $\therefore s = \frac{\text{sine of } z}{2 \text{ cos. of } z}$.

Scholium 1. As the two first theorems depend on the differences of the coefficients of the immediate terms or omitting none, the two last on the differences of the coefficients of the alternate terms or omitting one term; so we may give theorems for the differences of the coefficients of

[te]rms, omitting 2, 3, &c. terms; in fact, if r be a whole [nu]mber, and the terms of the series be all positive, or any now positive and negative by sets, provided the same signs return in the same order after every set, consisting of r number of terms; by continually multiplying by 2 sine of $\frac{r}{2}qz$, we shall get new series by taking the differences of the coefficients of every term and the rth succeeding term beginning with r number of noughts; except indeed that the coefficients of the terms will sometimes have the order of signs interrupted, namely, when a greater value is to be subtracted from a less.

But if every set should have the same order to signs contrary to those in the set immediately preceding, and consequently every set omitting one set continually, have the same order of signs, then by continually multiplying by 2 cos. of $\frac{r}{2}qz$, we shall get new series by taking the differences of the coefficients of any term and the rth term from it.

Scholium II. We may by the methods above not only find the valuation of infinite series, but likewise of finite series.

Example 1, Required the sum of the r first terms of the series, cos. of $nz +$ cos. of $\overline{n+q}.z +$ cos. of $\overline{n+2q}.z$ &c.

The series *ad infinitum* may be written thus, cos. of $nz +$ cos. of $\overline{n+q}.z +$ cos. of $\overline{n+2q}.z$ - - - - $+$ cos. of $\overline{n+\overline{r-1}.q}z$ $+$ cos. of $\overline{n+rq}.z +$ cos. of $\overline{n+\overline{r+1}.q}.z +$ &c. *ad infinitum*, from which if we take cos. of $\overline{n+rq}.z +$ cos. of $\overline{n+\overline{r+1}.q}.z$ $+$ cos. of $\overline{n+\overline{r+2}.q}.z$ &c. *ad infinitum*, we shall have the required sum; the first of these by *Example* 2, *Theorem* I. $= -\frac{\text{sine of } \overline{n-\frac{1}{2}q}\, z}{2 \text{ sine of } \frac{1}{2}qz}$, and the second by the same, by merely

writing $n+rq$ in the room of n, is equal to $-\frac{\text{sine of } \overline{n+r-\frac{1}{2}}.q.z}{2 \text{ sine of } \frac{1}{2}qz}$, consequently the sum of the r first terms $=$ $\frac{\text{sine of } \overline{n+\overline{r-\frac{1}{2}}q}.z - \text{sine of } \overline{n-\frac{1}{2}q}.z}{2 \text{ sine of } \frac{1}{2}qz}$.

Cor. 1. If $n=q=1$, and rz the whole circumference of the circle, we shall have cos. of $z+$ cos. of $2z+$ cos. of $3z$ - - - - $+$ cos. of $rz=\frac{\text{sine of } \overline{360^\circ+\overline{n-\frac{1}{2}}q}.z - \text{sine of } \overline{n-\frac{1}{2}q}.z}{2 \text{ sine of } \frac{1}{2}qz}=0$, a theorem said to be used by Le Gendre in his inscription of a polygon of 17 sides; and if we have $rqz=$ to the whole circumference, we likewise have in general cos. of $nz+$ cos. of $\overline{n+q}.z$ - - - - $+$ cos. of $\overline{n+\overline{r-1}.q}.z=0$, and if $n=\frac{1}{2}q$, we have in general cos. of $nz+$ cos. of $3nz+$ cos. of $5nz+$ &c. - - - - - cos. of $\overline{2r-1}.nz=\frac{\text{sine of } 2rnz}{2 \text{ sine of } nz}$.

Example 2, Required the sum of the series, cos. of $nz-$ cos. of $\overline{n+q}.z+$ cos. of $\overline{n+2q}.z$ - - - - - $\pm$ cos of $\overline{n+\overline{r-1}.q}.z$ the upper sign to be taken if r be odd, and the under sign if even.

The series is evidently the difference between the series cos. of $nz-$ cos. of $\overline{n+q}.z+$ &c. *ad infinitum* and $\mp$ cos. of $\overline{n+rq}.z\pm$ cos. of $\overline{n+\overline{r+1}.q}.z$ &c. *ad infinitum*, by proper substitution in *Example* 1, *Theorem* II. we have their respective sums $\frac{\text{cos. of } \overline{n-\frac{1}{2}q}.z}{2 \text{ cos. of } \frac{1}{2}qz}$ and $\mp\frac{\text{cos. of } \overline{n+\overline{r-\frac{1}{2}}.q}.z}{2 \text{ cos. of } \frac{1}{2}qz}$, and the difference $=$ $\frac{\text{cos. of } \overline{n-\frac{1}{2}q}.z\pm \text{cos. of } \overline{n+\overline{r-\frac{1}{2}}q}.z}{\text{cos. of } \frac{1}{2}qz}$, the upper sign to be taken if r be odd and the under if even.

Example 3, Required the sum of the r first terms of the series,

t^2 cos. of $pz - \overline{t+v}|^2$ cos. of $\overline{p+q}.z + \overline{t+2v}|^2$ cos. of $\overline{p+2q}.z -$ $\overline{t+3v}|^2$ cos. of $\overline{p+3q}.z$ &c.

Using *Theorem* II. to find the sum *ad infinitum*, and expanding the coefficients, we have,

$$\left.\begin{array}{l} a,\ b,\ c,\ d,\ \&c. = t^2,\ t^2+2tv+v^2,\ t^2+4tv+4v^2,\ t^2+6tv+9v^2,\\ \qquad\qquad\qquad\qquad t^2+8tv+16v^2,\ \&c.\\ a,\ a',\ b',\ c',\ \&c. = t^2,\quad 2tv+v^2,\quad 2tv+3v^2,\quad 2tv+5v^2,\\ \qquad\qquad\qquad\qquad 2tv+7v^2,\ \&c.\\ a,\ a'',\ b'',\ c'',\ \&c. = t^2,\ -t^2+2tv+v^2,\quad 2v^2,\quad 2v^2,\\ \qquad\qquad\qquad\qquad 2v^2,\ \&c.\\ a,\ a''',\ b''',\ c''',\ \&c. = t^2,\ -2t^2+2tv+v^2,\ t^2-2tv+v^2,\quad 0,\\ \qquad\qquad\qquad\qquad 0,\ \&c. \end{array}\right\}.$$

Therefore s the sum *ad infinitum* $= [t^2$ cos. of $\overline{p-\frac{3}{2}q}.z + \overline{2t^2-2tv-v^2}.$ cos. of $\overline{p-\frac{1}{2}q}.z + \overline{t-v}|^2$ cos. of $\overline{p+\frac{1}{2}q}.z] \div \overline{2 \text{ cos. of } \frac{1}{2}qz}|^3$, but the sum of r first terms of the series is evidently equal to the sum *ad infinitum* $\pm$ the sum of the series $\overline{t+rv}|^2$ cos. of $\overline{p+rq}.z - \overline{t+\overline{r+1}.v}|^2$ cos. of $\overline{p+\overline{r+1}.q}.z +$ &c. *ad infinitum*, which is found from the last by writing $t+rv$ for t, and $p+rq$ for p, to be $[\overline{t+rv}|^2$ cos. of $\overline{p+\overline{r-\frac{3}{2}}q}.z + \overline{2t^2+\overline{2r-1}.2tv + \overline{2r^2-2r-1}.v^2}$ cos. of $\overline{p+\overline{r-\frac{1}{2}}q}.z + \overline{t+\overline{r-1}.v}|^2.$ cos. of $\overline{p+\overline{r+\frac{1}{2}}.q}.z] \div \overline{2 \text{ cos. of } \frac{1}{2}qz}|^3$, which added to, or subtracted from, the value above, according as r is odd or even, gives the sum of r first terms of the original series.

Cor. If $z=0$, the cosine of any multiple of z will be equal to 1, therefore the sum of r first terms of $t^2 - \overline{t+v}|^2 + \overline{t+2v}|^2$ &c. will be equal to $\frac{t^2+2t^2-2tv-v^2+\overline{t-v}|^2}{8} \pm$

$\frac{\overline{t+rv}|^2 + 2t^2 + \overline{2r-1}.2tv + \overline{2r^2-2r-1}.v^2 + \overline{t+\overline{r-1}.v}|^2}{8} = \frac{t^2-tv}{2} \pm$

$\frac{t^2+\overline{2r-1}.tv+\overline{r^2-r}.v^2}{2}$ + or — to be taken according as r is odd or even, that is, $t^2+\overline{r-1}.tv+\frac{r^2-r}{2}v^2$ if r be odd, and $-rtv-\overline{\frac{r^2-r}{2}}.v^2$ if even. And thus we might proceed to the discovery of an infinite variety of theorems relative to the sines and cosines contained between any two limits in a circle, and the consequent inferences, the method being capable of a very extensive application; but rather than tire the reader's patience with what he may effect himself from what has been already said, if there should otherwise have been any difficulty, I shall propose

Theorem V.

If we have the sum of the series, a sine of $pz+b$ sine of $\overline{p+q}.z+c$ sine of $\overline{p+2q}.z+d$ sine of $\overline{p+3q}.z$ &c. expressed generally in terms of p, q, and z, we may find the sum of the series, a cos. of $pz+b$ cos. of $\overline{p+q}.z+c$ cos. of $\overline{p+2q}.z+d$ cos. of $\overline{p+3q}.z$ &c. expressed generally in terms of p, q, and z, and the contrary.

For if we put $90°+pz$ for pz in the series, and in the expression for its sum, we shall have instead of the sum of the series, a sine of $pz+b$ sine of $\overline{p+q}.z+c$ sine of $\overline{p+2q}.z$ &c., the sum of the series, a. sine of $\overline{90°+pz}+b$. sine of $\overline{90°+\overline{p+q}.z}$ $+c$ sine of $\overline{90°+\overline{p+2q}.z}$ &c. or because the sine of an arc is equal to the sine of $\overline{180°-\text{that arc}}$, we shall have the sum of the series a. sine of $\overline{90°-pz}+b$. sine of $\overline{90°-\overline{p+q}.z}$ &c. or its equal, a. cos. of $pz+b$. cos. of $\overline{p+q}.z+c$. cos. of $\overline{p+2q}.z$ &c. which is the first part of the theorem; and by following the

steps backwards, and substituting $pz-90^\circ$ for pz throughout we get the second part Q. E. D.

This theorem evidently supposes that the *functional* values of pz are distinct in the general expression for the sum of the series, before the substitution takes place, which may not be the case if p has any particular value, or even if p, q, and z have any relation to each other.

Theorem VI.

Given the sum of the series, a sine of $\pi z+.b$ sine of $\overline{\pi+\varkappa}.z$ $+c$ sine of $\overline{\pi+2\varkappa}.z+d$ sine of $\overline{\pi+3\varkappa}.z+$ &c. and likewise of a cos. of $\pi z+b$ cos. of $\overline{\pi+\varkappa}.z+c$ cos. of $\overline{\pi+2\varkappa}.z+$ &c. expressed generally and distinctly in terms of z for any particular values of π and $\varkappa$, except $\varkappa=0$, π and $\varkappa$ having the same value in both series; there will likewise be given the sum of the series, a sine of $pz+b$ sine of $\overline{p+q}.z+c$ sine of $\overline{p+2q}.z$ &c. and likewise of, a cos. of $pz+b$ cos. of $\overline{p+q}.z+c$ cos. of $\overline{p+2q}.z$ &c generally and distinctly in terms of p, q, and z.

For, calling the first series A and the second B, and putting $z=\frac{qx}{\varkappa}$, we have by substitution,

$$a \text{ sine of } \frac{q\pi}{\varkappa}x+b.\text{sine of } \overline{\frac{q\pi}{\varkappa}+q}.x+c.\text{ sine of } \overline{\frac{q\pi}{\varkappa}+2q}.x+d.\text{ sine of } \overline{\frac{q\pi}{\varkappa}+3q}.x+\text{ \&c.}=\text{A, and}$$

$$a \text{ cos. of } \frac{q\pi}{\varkappa}x+b.\text{cos. of } \overline{\frac{q\pi}{\varkappa}+q}.x+c.\text{cos. of } \overline{\frac{q\pi}{\varkappa}+2q}.x+d.\text{ cos. of } \overline{\frac{q\pi}{\varkappa}+3q}.x+\text{ \&c.}=\text{B.}$$

A and B being now expressed in general terms of q and x, and particular values; multiply the first by, 2 cos. of $\overline{p-\frac{q\pi}{\varkappa}}.x$

and the second by 2 sine of $\overline{p - \frac{q\pi}{\varkappa}}\,.\,x$ by means of lemma No. II. and we get, a sine of $px - a$ sine of $\overline{p - \frac{2q\pi}{\varkappa}}.x + b$ sine of $\overline{p+q}.x - b$ sine of $\overline{p - \frac{2q\pi}{\varkappa} - q}\,.\,x + c$ sine of $\overline{p+2q}.x - c$ sine of $\overline{p - \frac{2q\pi}{\varkappa} - 2q}\,.\,x$ &c. $= 2$A cos. of $\overline{p - \frac{q\pi}{\varkappa}}\,.\,x$, and a sine of $px + a$ sine of $\overline{p - \frac{2q\pi}{\varkappa}}\,.\,x + b$ sine of $\overline{p+q}.x + b$ sine of $\overline{p - \frac{2q\pi}{\varkappa} - q}\,.\,x + c$ sine of $\overline{p+2q}\,.x + c$ sine of $\overline{p - \frac{2q\pi}{\varkappa} - 2q}\,.\,x$ &c. $= 2$B sine of $\overline{p - \frac{q\pi}{\varkappa}}\,x$; consequently, adding these two together, we have by dividing by 2, a sine of $px + b$ sine of $\overline{p+q}\,.\,x + c$ sine of $\overline{p+2q}\,.\,x$ &c. $=$ A cos. of $\overline{p - \frac{q\pi}{\varkappa}}\,.\,x +$ B sine of $\overline{p - \frac{q\pi}{\varkappa}}\,.\,x$, expressed generally and distinctly in terms of p, q, and x, the equation will therefore remain if we put z in the place of x throughout, and therefore the sum of the series, a sine of $pz + b$ sine of $\overline{p+q}.z$ &c. is given expressed generally in terms p, q, and of z, which is the first part of the theorem.

Again, by multiplying the series, a sine of $\frac{q\pi}{\varkappa}\,x + b$ sine of $\overline{\frac{q\pi}{\varkappa} + q}\,.\,x$ &c. $=$ A, by 2 sine of $\overline{p - \frac{q\pi}{\varkappa}}\,x$, by means of lemma No. I. and the series, a cos. of $\frac{q\pi}{\varkappa}\,x +$ cos. of $\overline{\frac{q\pi}{\varkappa} + q}\,.x$ &c. $=$ B, by 2 cos. of $\overline{p - \frac{q\pi}{\varkappa}}\,x$ by means of lemma No. II. we shall have a cos. of $\overline{p - \frac{q\pi}{\varkappa}}.\,x - a$ cos. of $px + b$ cos. of $\overline{p - \frac{2q\pi}{\varkappa} - q}\,.\,x - b$ cos. of $\overline{p+q}\,.\,x +$ cos. of $\overline{p - \frac{2q\pi}{\varkappa} - 2q}\,.x - c$ cos. of $\overline{p+2q}\,.\,x +$ &c. $= 2$A cos. of $\overline{p - \frac{q\pi}{\varkappa}}\,x$, and a cos. of $\overline{p - \frac{q\pi}{\varkappa}}\,.\,x + a$ cos. of $px + b$ cos. of $\overline{q - \frac{2q\pi}{\varkappa} - q}.x + b$ cos. of $\overline{p+q}.x + c$ cos. of $\overline{q - \frac{2q\pi}{\varkappa} - 2q}$

$.x+c$ cos. of $\overline{p+2q}.x+$ &c. $=2\text{B}$ cos. of $\overline{p-\frac{q\pi}{\varkappa}}x$; half the difference of these two series gives,

a cos. of $px+b$ cos. of $\overline{p+q}.x+c$ cos. of $\overline{p+2q}.x+$ &c. $=\text{B}$ cos. of $\overline{p-\frac{q\pi}{\varkappa}}.x-\text{A}$ sine of $\overline{p-\frac{q\pi}{\varkappa}}.x$ expressed generally and distinctly in terms of p, q, and x; and consequently by writing z for x throughout, we have the sum of the series, a cos. of $pz+b$ cos. of $\overline{p+q}.z+c$ cos. of $\overline{p+2q}.z+$ &c. expressed generally and distinctly in terms z, p, and q. Q. E. D.

Cor. I. It is evident that p and q may be taken any numbers either positive or negative, but $\varkappa$ ought not to be equal to o, for we could not then effect the substitution $z=\frac{qx}{\varkappa}$.

Cor. II. Putting, a cos. of $pz+b$ cos. of $\overline{p+q}.z$ &c. $=\text{P}$, and a sine of $pz+b$ sine of $\overline{p+q}.z$ &c. $=\text{Q}$, and also B′ and A′ for the values that B and A become, by writing z for x in those values, that is, z for $\frac{\varkappa z}{q}$ in the given expressions B and A we shall have $\text{P}=\text{B}'$ cos. of $\overline{p-\frac{q\pi}{\varkappa}}z-\text{A}'$ sine of $\overline{p-\frac{q\pi}{\varkappa}}.z$, and $\text{Q}=\text{B}'$ sine of $\overline{p-\frac{q\pi}{\varkappa}}.z+\text{A}'$ cos. of $\overline{p-\frac{q\pi}{\varkappa}}.z$.

Cor. III. Hence we may again prove, that if we have the sum of the series, a sine of $pz+b$ sine of $\overline{p+q}.z+$ &c. expressed generally in terms of p, q, and z, we may find the series, a cos. of $pz+b$. cos. of $\overline{p+q}.z+$ &c. expressed generally in terms of p, q, and z, and the contrary. For having the sum of the first by writing π for p, $\varkappa$ for q, we shall have the sum of the series, a sine of $\pi z+b$ sine of $\overline{\pi+\varkappa}.z+$ &c. $=\text{A}$, expressed by z, and particular values; in which writing $\frac{\varkappa z}{q}$ for z, we get A′, therefore having A′ and Q, we may, by

help of *Cor.* II. find P in terms of p, q, and z, and particular values, namely, the sum of the series a cos. of $pz+b$ cos. of $\overline{p+q}.z$ &c. and in a similar manner the contrary is proved. Q. E. D.

Theorem VII.

If we have the sum of the series, a sine of $pz+b$ sine of $qz+c$ sine of $rz+$ &c. expressed generally by z; we have likewise the sum of the series, a cos. of px . sine of $py+b$ cos. of qx . sine of $qy+c$ cos. of rx sine of ry &c. expressed generally by x and y. And if we have the sum of the series, a cos. of $pz+b$ cos. of $qz+c$ cos. of rz &c. expressed generally by z; we have likewise the sum of the series, a cos. of px . cos. of $py+b$ cos. of qx . cos. of $qy+c$ cos. of rx . cos. of ry &c.; and also the sum of the series, a sine of px . sine of $py+b$ sine of qx . sine of $qy+c$ sine of rx sine of ry &c. expressed generally in terms of x and y.

First; if we have the sum of the series, a sine of $pz+b$ sine of qz &c. expressed in terms of z, by writing $x+y$ in the room of z throughout, we shall have the sum of the series, a sine of $p.\overline{x+y}+b$ sine of $q.\overline{x+y}+c$ sine of $r.\overline{x+y}$ &c. expressed in terms of x and y and in like manner by writing $x-y$ for z we shall have the sum of the series, a sine of $p.\overline{x-y}+b$ sine of $q.\overline{x-y}+c$ sine of $r.\overline{x-y}$ &c. expressed in terms of x and y, therefore the half difference of these two, that is, $a.\frac{\text{sine of } p.\overline{x+y}-\text{ sine of } p.\overline{x-y}}{2}+b.\frac{\text{sine of } q.\overline{x+y}-\text{ sine of } q.\overline{x-y}}{2}$ $+c.\frac{\text{sine of } r.\overline{x+y}-\text{ sine of } r.\overline{x-y}}{2}$ &c. or its equal by lemma No. II, a cos. of px. sine of $py+b$ cos. of qx. sine of $qy+c$ cos. of rx.

sine of ry + &c. will likewise be expressed generally in terms of x and y, which is case the first.

Again, if we have the sum of the series a cos. of $pz + b$ cos. of qz &c., expressed generally by z; by writing $x-y$ throughout for z we shall have the sum of the series a.cos. of $p.\overline{x-y} + b$ cos. of $q.\overline{x-y} + c$ cos. of $r.\overline{x-y}$ &c. expressed generally by x and y and by writing $\overline{x+y}$ for z throughout, we shall have the sum of of the series a cos. of $p.\overline{x+y} + b$ cos. of $q.\overline{x+y} + c$ cos. of $r.\overline{x+y}$ &c. expressed generally in terms of x and y, and consequently the half sum of the two which by lemma No. III. is equal to a cos. of px. cos. of py + b. cos. of qx. cos. of $qy + c$ cos. of rx. cos. of ry &c. will be expressed generally in terms of x and y; and the half difference of the two which by lemma No. I. is equal to, a sine of px. sine of $py + b$ sine of qx. sine of $qy + c$ sine of rx. sine of ry &c. will likewise be expressed generally in terms of x and y. Q. E. D.

Corollary. From the sum of the series, a sine of $pz + b$ sine of qz + &c. having obtained the sum of the series, a cos. of of px. sine of $py + b$ cos. of qx. sine of qy &c. if a' be put for a cos. of px, b' for b cos. of qx, c' for c cos. of rx, &c. this series will be reduced to a' sine of $py + b'$ sine of $qy + c'$ sine of ry &c. which is of the first form of this theorem, and consequently from it may be deduced the sum of the series a' cos. of pw. sine of $pv + b'$ cos. of qw. sine of $qv + c$ cos. of rw. sine of rv &c. and therefore its equal the sum of the series a cos. of pw. cos. of px. sine of $pv + b$ cos. of qw. cos. of qx. sine of $qv + c$ cos. of rw. cos. of rx. sine of rv + &c. in terms of v, w, and x, but if a' had been put for a sine of py, b' for

b sine of qy &c.; we should have had the series reduced to the form a' cos. of $py + b'$ cos. of qy &c. which is of the 2d form of the theorem, and consequently from it is deduced the sum of each of the series, 1st. a' cos. of pw . cos. of $pv + b'$ cos. of qw . cos. of qv &c. that is, of the series a cos. of pw . cos. of pv . sine of $px + b$ cos. of qw . cos. of qv . sine of qx &c. in terms of v, w, and x, which is indeed similar in form to the series found by the other substitution; and 2d. the sum of the series a' sine of pw . sine of $pv + b'$ sine of qw . sine of qv &c. or its equal the sum of the series a sine of pw . sine of px . sine of $pv + b$ sine of qw . sine of qx . sine of qv + &c. in terms of v, w, and x. And in a similar manner, from the sum of the series a cos. of $pz + b$ cos. of qz &c. having found the sum of the series a cos. of px . cos. of $py + b$ cos. of qx . cos. of qy &c. we may find the sum of the series a cos. of pw . cos. of pv . cos. of $px + b$ cos. of qw . cos. of qv . cos. of qx + &c. in terms of w, v, and x, and likewise the sum of the series a cos. of pw . sine of pv . sine of $px + b$ cos. of qw . sine of qv . sine of qx &c. And in a similar manner also may we proceed by degrees to more complicated cases.

Example 1. Because (from *Example* 1. *Scholium* 2. after *Example* 3. *Theorem* IV.) we have the sum of the r first terms of the series, cos. of nz + cos. of $\overline{n+q}\,.\,z$ + cos. of $\overline{n+2q}\,.\,z$ &c. = [sine of $\overline{n+r-\frac{1}{2}.q}\,.\,z$ — sine of $\overline{n-\frac{1}{2}q}\,.\,z$] : 2 sine of $\frac{1}{2}qz$: if $x-y$ and $x+y$ be written for z, then the half sum and half difference of the resulting expressions, by case 2 of this theorem, will give the r first terms of the series cos. of nx . cos. of ny + cos. of $\overline{n+q}\,.\,x$. cos. of

$\overline{n+q}\,.\,y$ &c. $= \dfrac{\text{sine of } \overline{n+\overline{r-\frac{1}{2}}\,.\,q}\,.\,\overline{x-y} - \text{sine of } \overline{n-\frac{1}{2}q}\,.\,\overline{x-y}}{4 \text{ sine of } \frac{1}{2}q\,.\,\overline{x-y}}$

$+ \dfrac{\text{sine of } \overline{n+\overline{r-\frac{1}{2}}q}\,.\,\overline{x+y} - \text{sine of } \overline{n-\frac{1}{2}q}\,.\,\overline{x+y}}{4 \text{ sine of } \frac{1}{2}q\,.\,\overline{x+y}}$; and the sum of the r first terms of the series sine of nx . sine of ny + sine of $\overline{n+q}\,.\,x$. sine of $\overline{n+q}\,.\,y$ + sine of $\overline{n+2q}\,.\,x$. sine of $\overline{n+2q}\,.\,y$ &c.

$= \dfrac{\text{sine of } \overline{n+\overline{r-\frac{1}{2}}q}\,.\,\overline{x-y} - \text{sine of } \overline{n-\frac{1}{2}q}\,.\,\overline{x-y}}{4 \text{ sine of } \frac{1}{2}q\,.\,\overline{x-y}} - [\text{sine of } \overline{n+\overline{r-\frac{1}{2}}\,.\,q}\,.\,\overline{x+y} - \text{sine of } \overline{n-\frac{1}{2}q}\,.\,\overline{x+y}] \div 4 \text{ sine of } \frac{1}{2}q\,.\,\overline{x+y}.$

Cor. If rx and ry be both multiples of the whole circumference the said two values will be equal to o.

Example 2. Because (from *Cor.* 1. *Example* 2. *Theorem* II.) we have sine of $pz - r$ sine of $\overline{p+q}\,.\,z + r\,.\,\frac{r+1}{2}$ sine of $\overline{p+2q}\,.\,z$ &c. $= \dfrac{\text{sine of } \overline{p-\frac{1}{2}qr}\,.\,z}{2 \cos. \text{ of } \frac{1}{2}qz|^r}$, we have by this theorem case 1, cos. of px . sine of $py - r$ cos. of $\overline{p+q}\,.\,x$. sine of $\overline{p+q}\,.\,y + r\,.\,\frac{r+1}{2}$ cos. of $\overline{p+2q}\,.\,x$. sine of $\overline{p+2q}\,.\,y$ &c. $= \dfrac{\text{sine of } \overline{p-\frac{1}{2}qr}\,.\,\overline{x+y}}{2\,.\,2 \cos. \text{ of } \frac{1}{2}q\,.\,\overline{x+y}|^r}$

$- \dfrac{\text{sine of } \overline{p-\frac{1}{2}qr}\,.\,\overline{x-y}}{2\,.\,2 \cos. \text{ of } \frac{1}{2}q\,.\,\overline{x-y}|^r}$. And because by the same cos. of $pz - r$ cos. of $\overline{p+q}\,.\,z + r\,.\,\frac{r+1}{2}$ cos. of $\overline{p+2q}\,.\,z$ &c. $= \dfrac{\cos. \text{ of } \overline{p-\frac{1}{2}qr}\,.\,z}{2 \cos. \text{ of } \frac{1}{2}qz|^r}$ we have by case 2 of this theorem cos. of px . cos. of $py - r$ cos. of $\overline{p+q}\,.\,x$. cos. of $\overline{p+q}\,.\,y + r\,.\,\frac{r+1}{2}$ cos. of $\overline{p+2q}\,.\,x$. cos. of $\overline{p+2q}\,.\,y$ &c. $= \dfrac{\cos. \text{ of } \overline{p-\frac{1}{2}qr}\,.\,\overline{x-y}}{2\,.\,2 \cos. \text{ of } \frac{1}{2}q\,.\,\overline{x-y}|^r} + \dfrac{\cos. \text{ of } \overline{p-\frac{1}{2}qr}\,.\,\overline{x+y}}{2\,.\,2 \cos. \text{ of } \frac{1}{2}q\,.\,\overline{x+y}|^r}$

and sine of px . sine of $py - r$ sine of $\overline{p+q}\,.\,x$. sine of $\overline{p+q}\,.\,y$ + &c. $= \dfrac{\cos. \text{ of } \overline{p-\frac{1}{2}qr}\,.\,\overline{x-y}}{2\,.\,2 \cos. \text{ of } \frac{1}{2}q\,.\,\overline{x-y}|^r} - \dfrac{\cos. \text{ of } \overline{p-\frac{1}{2}qr}\,.\,\overline{x+y}}{2\,.\,2 \cos. \text{ of } \frac{1}{2}q\,.\,\overline{x+y}|^r}$.

Cor. If in these we either put $qr - p$ or $\frac{1}{2}qr$ in the place of p, we shall get theorems for the rectangles of sine and cosines, rectangles of cosines and the rectangles of sines similar to those of *Cor.* II. and III. (respectively) *Example* 2. *Theorem* II. for the simple sines and cosines.

General Scholium.

It is necessary to obeserve, that there may be particular cases, in the summation of which these methods fail, and which, if not properly considered, may lead to great error, especially when new series are derived from those containing failing cases, by multiplying by fluxions, and finding the fluents of the expressions thence arising. For if the correction should happen to be sought from any of the failing cases, the summation of the new series might not only fail in the failing case of the primary expression, but in every other.

From *Example* 1. *Theorem* I. we have sine of pz + sine of $\overline{p+q}.z$ + sine of $\overline{p+2q}.z$ &c. $= \frac{\text{cos. of } \overline{p - \frac{1}{2}q}.z}{2 \text{ sine of } \frac{1}{2}qz}$; this when $z = 0$, will be sine of 0 + sine of 0 + sine of 0 &c. or $0 + 0 + 0$ &c. $= \frac{\text{cos. of } 0}{2 \text{ sine of } 0} = \frac{1}{2 \text{ sine of } 0}$; that is the sum of a series of noughts infinite, which is absurd. Again, in *Example* 2. *Theorem* I. *Cor.* I. cos. of nz + cos. of $3nz$ + cos. of $5nz$ &c. $= 0$, therefore if z be taken $= 0$ it will be $1 + 1 + 1$ &c. $= 0$ which ought to be infinite, and in *Cor.* II. z being $= 0$ we have $1 + 1 + 1 + 1$ &c. $= -\frac{1}{2}$.

In order to explain the reason of these absurdities, and to prevent the errors they may produce, it is necessary to con-

sider the subject more minutely, to which purpose *Scholium* II. at the end of *Theorem* IV. will afford great assistance: from that it appears, that the sum of the r first terms of the series cos. of $nz +$ cos. of $\overline{n+q}\,.z +$ cos. of $\overline{n+2q}\,.z$ &c. $=$ $\frac{\text{sine of } \overline{n+\overline{r-\frac{1}{2}q}}\,z - \text{ sine of } \overline{n-\frac{1}{2}q}.z}{2 \text{ sine of } \frac{1}{2}qz}$; and by similar means that the sum of the r first terms of the series sine of $nz+$ sine of $\overline{n+q}.z+$ sine of $\overline{n+2q}z+$ &c. $= \frac{-\text{cos. of } \overline{n+\overline{r-\frac{1}{2}q}}\,z + \text{cos. of } \overline{n-\frac{1}{2}q}\,z}{2 \text{ sine of } \frac{1}{2}qz}$: now it is plain that if qz were either equal to o or a multiple of 360°, sine of $\frac{1}{2}qz$ would be equal to o, and because r is a whole number, rqz would either be equal to o or a multiple of 360°, and consequently the sine of $\overline{n+\overline{r-\frac{1}{2}}.q}\,.z =$ sine of $\overline{n-\frac{1}{2}q}.z$ and the cosine of $\overline{n+\overline{r-\frac{1}{2}}.q}.z =$ cos. of $\overline{n-\frac{1}{2}.q}.z$, and therefore the sum of the series, cos. of $nz+$ cos. of $\overline{n+q}.z$ &c. $=$(when $qz = $o or some multiple of 360°) - - - - $\frac{\text{sine of } \overline{n-\frac{1}{2}q}.z - \text{ sine of } \overline{n-\frac{1}{2}q}.z}{0} = \frac{0}{0}$, and of sine of $nz+$ sine of $\overline{n+q}.z$ &c. $= \frac{0}{0}$ whatever r may, whether finite or infinite. Indeed the determining the value of $\frac{0}{0}$, depends on the value of r; but if qz be any thing but o or a multiple of 360°, the value of the sine or cosine of $\overline{n+\overline{r-\frac{1}{2}}q}.z$ will depend on the value of r, and may then be varied from positive to negative and from negative to positive, by merely increasing r, and consequently when r is infinite, there being no reason for its being positive rather than negative, or negative rather than positive it should be considered o; and therefore the sum of the infinite series, cos. of $nz+$ cos. of $\overline{n+q}\,.z$ &c. $= -\frac{\text{sine of } \overline{n-\frac{1}{2}q}.z}{\text{sine of } \frac{1}{2}qz}$ and of sine of $nz +$ sine of $\overline{n+q}\,.z$ &c. $=$

$\frac{\text{cos. of } \overline{n-\frac{1}{2}q}.z}{2 \text{ sine of } \frac{1}{2}qz}$ in every case, (the same as in *Example* to *Theorem* I.) except when $qz=0$ or some multiple of 360°; on account of there being something else to be taken into consideration, in that case. Again, it appears by *Example* 2 of the said scholium, that the sum of the r first terms of the series, cos. of nz — cos. of $\overline{n+q}.z+$ cos. of $\overline{n+2q}.z$ — &c. $=$ $\frac{\text{cos. of } \overline{n-\frac{1}{2}q}.z \pm \text{ cos. of } \overline{n+\overline{r-\frac{1}{2}}.q}.z}{2 \text{ cos. of } \frac{1}{2}qz}$, the upper or under sine to be taken according as r is odd or even. And by similar means the sum of the r first terms of the series, sine of nz— sine of $\overline{n+q}.z+$ sine of $\overline{n+2q}.z$— sine of $\overline{n+3q}.z$ &c. is found $=$ $\frac{\text{sine of } \overline{n-\frac{1}{2}q}.z \pm \text{ sine of } \overline{n+\overline{r-\frac{1}{2}}\,q}\,z}{2 \text{ cos. of } \frac{1}{2}qz}$, the upper sign to be taken if r is odd but the under if even. Here if $qz=180°$, or any odd multiple thereof, the cosine of $\frac{1}{2}qz$ will be $=0$; and if r be even at the same time the cos. of $\overline{n+\overline{r-\frac{1}{2}}.q}.z$ will be equal to the cosine of $\overline{n-\frac{1}{2}.q}.z$ and the sine of $n+r-\frac{1}{2}.q.z=$ sine of $\overline{n-\frac{1}{2}qz}$; but if r be odd we shall have the cos. of $n+\overline{r-\frac{1}{2}}.q.z$ $=$ — cos. of $\overline{n-\frac{1}{2}q}.z$ and sine of $\overline{n+\overline{r-\frac{1}{2}}.q}.z=$ — the sine of $\overline{r-\frac{1}{2}q}.z$; consequently by substituting these values in the above expressions for the sum, due regard being had to the signs, we shall find that, whatever r be, the sum of either series will be expressed by $\frac{0}{0}$: but if z be any other value it appears that $\pm$ the sine or cosine of $\overline{n+\overline{r-\frac{1}{2}}.q}.z$ depends on the value of r, and may be either positive or negative, by varying r; and consequently should as above when r is infinite be considered $=0$. And the sum of the series, sine of

$nz -$ sine of $\overline{n+q}.z +$ &c. will be $= \frac{\text{sine of } \overline{n-\frac{1}{2}q}\, z}{2 \text{ cos. of } \frac{1}{2}qz}$, and of cos. of $nz -$ cos. of $\overline{n+q}.z +$ &c. $= \frac{\text{cos. of } \overline{n-\frac{1}{2}q}\, z}{2 \text{ cos. of } \frac{1}{2}qz}$, as in *Example* 1, *Theorem* II. except when $qz =$ some odd multiple of 180°, something else being in that case to be taken into consideration; and thus are we to reason, in the failing cases of other expressions: but by the common rules for finding the value of an expression when the denominator and numerator vanish, we may find the value even in the failing cases; thus by dividing the fluxion of the numerator by the fluxion of the denominator in the expressions $\frac{-\text{ cos. of } \overline{n+r-\frac{1}{2}q}.z + \text{ cos. of } \overline{n-\frac{1}{2}q}\, z}{2 \text{ sine of } \frac{1}{2}qz}$ and $\frac{\text{sine of } \overline{n+r-\frac{1}{2}\,q}.z - \text{ sine of } \overline{n-\frac{1}{2}q}\, z}{2 \text{ sine of } \frac{1}{2}qz}$, and then making $qz = 0$, or some multiple of 360°, we shall get simply, 0 for the sum of the r first terms of the series sine of $nz +$ sine of $\overline{n+q}.z +$ &c., and r for the sum of the r first terms of the series cos. of $nz +$ cos. of $\overline{n+q}.z +$ &c. when $z = 0$ or some multiple of 360°, that is, 0 for the sum of the r first terms of the series, $0+0+0$ &c. and r for the sum of the r first terms of the series, $1+1+1$ &c. which is self-evident. And thus may we proceed in other expressions when the sum of r terms can be obtained by a general value.

That these things should happen as above described, is likewise evident, from the investigations of the theorems; for in *Theorem* I. for instance, we have $s^{\pi} = \pm\, \overline{2 \text{ sine of } \frac{1}{2}qz}|^{\pi-1}$ or $\pm\, s\,.\,\overline{2 \text{ sine of } \frac{1}{2}qz}|^{\pi}$, π being a positive whole number; therefore if the sine of $\frac{1}{2}qz$ be $= 0$, which will happen when $qz = 0$ or some multiple of 360°, it is plain that we should have

$s^{\varpi} = \pm s' \times 0$, or $\pm s \times 0$, and consequently s^{ϖ} ought likewise to come out equal to 0, and therefore s, would be $= \frac{0}{0}$; and consequently when s^{ϖ} in that case does not come out $= 0$, it is certain that there must have been something neglected: and to shew how this may happen, we observe that since *Theorem* I. and II. require the differences of the coefficients of every term and the next succeeding term to be taken, it is evident that the last term will have nothing to be taken from, and will consequently remain through every new series; in consequence of which there will be terms of the form Δ. sine or cosine of $\overline{\varpi + qr} . z$, (in which r is a whole number and infinite, the number of terms of the series being infinite,) whose coefficient Δ will never be $= 0$ unless the series $a, b, c,$ &c. be converging: these terms are unavoidably omitted, by reason of their place being at an infinite distance, and can consequently never be arrived at; but still unless it be equal to 0, it should not be omitted; which it cannot be unless, either in the above mentioned circumstance of the series $a, b, c,$ &c. being converging, or when the terms of the series of sines or cosines, are continually changing their signs, for different values of r; which it will always do when qz is not equal to 0 or some multiple of 360°; provided the coefficients $a, b, c,$ &c. are all affirmative: and consequently the said terms may be omitted in every such case, there being no reason for taking one sign rather than the other: but if qz were equal to 0 or some multiple of 360°, since Δ. sine or cosine of $\overline{\varpi + qr} . z$ will then be simply Δ. sine or cos. of pz, and therefore if the same sign whatever r may be, when $a, b, c, d,$ &c. ----- to Δ, have all the same signs; and consequently cannot

be then neglected unless in the case above mentioned of a, b, c, &c being converging, in which circumstance it will have no failing case: but had the coefficients a, b, c, d, &c. been alternately $+$ and $-$, the failing case would not happen when $qz = 0$ or a multiple of $360°$, for then there being no reason for taking Δ of one sign rather than the other, it should therefore be taking equal to 0; but it will happen when sine or cos. of $\overline{\varpi + qr}.z$ is alternately positive and negative, by continually increasing r by 1: for then the coefficients of the terms of the form, sine or cos. of $\overline{\varpi + qr}.z$ being alternately positive and negative; and likewise the terms themselves alternately positive and negative, the whole values resulting from them will have the same determinate sign, and this will be when $qz = 180°$ or some multiple thereof. And if a, b, c, d, &c. be positive and negative according to some other law, the failing cases may be found by the like reasoning; which is likewise applicable to the other theorems.

These remarks pave the way to the correction of fluents necessary in the application of the doctrine of fluxions to these series.

1. In *Example* 2, *Theorem* I. if for nz we write $k+z$, and for, q we write 1, we shall have cos. of $\overline{k+z}$ $+$ cos. of $\overline{k+2z}$ $+$ cos. of $\overline{k+3z}$ &c. $= -\frac{\text{sine of } \overline{k+\frac{1}{2}z}}{2 \text{ sine of } \frac{1}{2}z} = -$ sine of $k . \frac{\text{cos. of } \frac{1}{2}z}{2 \text{ sine of } \frac{1}{2}z} - \frac{\text{cos. of } k . \text{sine of } \frac{1}{2}z}{2 \text{ sine of } \frac{1}{2}z} = -\frac{\text{sine of } k . \text{ cos. of } \frac{1}{2}z}{2 \text{ sine of } \frac{1}{2}z} - \frac{\text{cos. of } k}{2}$. Multiply both sides by $\dot{z}$, and find the consequent fluents, and we shall have sine of $\overline{k+z} + \frac{\text{sine of } \overline{k+2z}}{2} + \frac{\text{sine of } \overline{k+3z}}{3}$ &c. $=$ fluent of $[-\frac{\dot{z}}{2}$ sine of $k . \frac{\text{cos. of } \frac{1}{2}z}{\text{sine of } \frac{1}{2}z} - \frac{\text{cos. of } k}{2} \dot{z}]$, which because $\frac{\dot{z}}{2}$ cos. of

$\frac{1}{2}z$, is equal to the fluxion of sine of $\frac{1}{2}z$, is equal to the fluent of $\left[- \text{sine of } k . \frac{\text{fluxion of sine of } \frac{1}{2}z}{\text{sine of } \frac{1}{2}z} - \frac{\text{cos of } k}{2} . \dot{z}\right] = -$ sine of k. log. sine of $\frac{1}{2}z - \frac{\text{cos of } k}{2} . z +$ a correction: now this correction must not be sought when $z = 0$ or a multiple of $360°$: for in that case from what has been just now said, the primary equation fails, or rather there is a supplemental value only then to be prefixed; therefore the easiest method which offers, is when $z = 180°$, we then have the sine of $\overline{k+z} = -k$, sine of $\overline{k+2z} =$ sine of k, sine of $\overline{k+3z} = -$ sine of k &c. and sine of $\frac{1}{2}z = 1$, consequently putting Q for $\frac{1}{4}$ of the periphery of a circle whose radius one, the expression will become $-$ sine $k + \frac{\text{sine of } k}{2} - \frac{\text{sine of } k}{3} + \frac{\text{sine of } k}{4}$ &c. or sine of $k \times$ log. of $\frac{1}{2} =$ $-$ cos. of $\overline{k} . Q +$ correction $\therefore$ correction $=$ sine of k. log. of $2 +$ cos. of $\overline{k} . Q$; which correction being prefixed we have, sine of $\overline{k+z} + \frac{\text{sine of } \overline{k+2z}}{2} + \frac{\text{sine of } \overline{k+3z}}{3}$ &c. $=$ sine of $\overline{k} \times$ log. of $\frac{1}{2 \cdot \text{sine of } \frac{1}{2}z} + \overline{Q - \frac{z}{2}} \times$ cos. of k: which is only true whilst z is between 0 and $360°$; for though our primary equation fails only when z is 0 or some multiple of $360°$, and is true in every other case, whatever z may be, whether more or less than $360°$; still it cannot be so in this derivative equation: for suppose K to be the said supplemental value, which is equal to 0 in every other case but that mentioned above, the derivative expression, will in that case contain the supplement, the fluent of $K . \dot{z}$ producing a correction which will remain when it is once introduced, though K may afterwards vanish, namely, when z becomes neither 0, nor any multiple of $360°$

and therefore every time z becomes, by flowing, any multiple of $360°$; K being introduced, it will introduce an additional correction, which will remain afterwards.

If this equation be multiplied by $\dot{z}$ and the fluent be again taken, we shall have $\frac{\text{cos. of } \overline{k+z}}{1} + \frac{\text{cos. of } \overline{k+2z}}{4} + \frac{\text{cos. of } \overline{k+3z}}{9}$ &c. $=$ — fluent of [sine of $\overline{k}$. $\dot{z}$ log. of $\frac{1}{2 \text{ sine of } \frac{1}{2}z} + Q - \frac{z}{2} . \dot{z}$ cos. of k] which fluent is easily found by infinite series, but if k be $= 0$ we shall have cos. of $k = 1$, and the fluent $= Qz - \frac{z^2}{4}$ independent of the correction, that is cos. of $z + \frac{\text{cos. of } 2z}{2^2} + \frac{\text{cos of } 3z}{3^2}$ &c. $= A - Qz + \frac{z^2}{4}$, A standing for the correction: if z be $=$ to the arc of $90°$ or Q, we shall have cos. of $z = 0$, cos. of $2z = -1$, cos. of $3z = 0$, cos. of $4z = 1$ &c. therefore we shall have by substitution $-\frac{1}{2^2} + \frac{1}{4^2} - \frac{1}{6^2} + \frac{1}{8^2}$ &c. $= A - Q^2 + \frac{Q^2}{4} = A - \frac{3}{4} Q^2$, but if in the equation z be taken $= 180°$ or $2Q$, we shall have cos. of $z = -1$, cos. of $2z = +1$, cos. of $3z = -1$, &c. &c. $\therefore -1 + \frac{1}{2^2} - \frac{1}{3^2} +$ &c. $= A - 2Q^2 + Q^2$ or $A - Q^2$, which series being the same as the other series when multiplied by 4, we have $A - Q^2 = 4A - 3Q^2 \therefore A = \frac{2}{3} Q^2 \therefore 1 - \frac{1}{2^2} + \frac{1}{3^2} -$ &c. $= Q^2 - A = \frac{Q^2}{3}$, and cos. of $z + \frac{\text{cos. of } 2z}{4} + \frac{\text{cos. of } 3z}{9}$ &c. $= \frac{2}{3} Q^2 - Qz + \frac{z^2}{4}$. It is remarkable that this equation is true, not only when the equation from which it is derived is true, but likewise when $z = 0$ or $360°$ in which that fails, and that the correction might have been sought in those cases had this circumstance been known. Multiply this again by $\dot{z}$, and find the fluent, and we

have sine of $z + \frac{\text{sine of } 2z}{8} + \frac{\text{sine of } 3z}{3^3}$ &c. $= \frac{2}{3} Q^2 z - \frac{Qz^2}{2} + \frac{z^3}{12}$ and this requires no correction whilst z is from 0 to 360 inclusively of both; this is evident at first sight, as we are not now obliged as before to avoid correcting when $z = 0$ or $360°$, as the equation from which this is derived does not fail in those cases.

2. If in the equation sine of nz — sine of $\overline{n+q}.z.$ + sine of $\overline{n+3q}z$ — &c. $= \frac{\text{sine of } \overline{n - \frac{1}{2}q}z}{2 \text{ cos. of } \frac{1}{2}qz}$, failing when $qz = 180°$ or an odd multiple thereof, we put $n = 1$, $q = 2$ we have sine of z — sine of $3z$ + sine of $5z$ &c. $= 0$, failing when $z = 90°$ or any odd multiple thereof; if we multiply this by $\dot{z}$ and find the fluent we shall have cos. of $z - \frac{\text{cos. of } 3z}{3} + \frac{\text{cos of } 5z}{5}$ — &c. = correction, which must not be sought when $z = 90°$, or odd multiple thereof; if it be sought when $z = 0$, we shall have it $= 1 - \frac{1}{3} + \frac{1}{5}$ &c. $= \frac{1}{2} Q$, which will answer whilst z is exclusively from $-90°$ to $+90°$. If the correction had been sought when $z = 180°$ we should have it $= -1 + \frac{1}{3} - \frac{1}{5}$ &c. $= -\frac{1}{2} Q$, answering whilst z is from $90°$ to $270°$.

3. Again, from *Cor.* 1. *Example* 2. *Theorem* I. cos. of z + cos. of $3z$ + cos. of $5z$ + &c. is equal to 0, failing (from above) when $qz = 0$ or a multiple of $360°$, and therefore when $z = 0$ or a multiple of $180°$: if we multiply by the $\dot{z}$ and take the fluent we have sine of $z + \frac{\text{sine of } 3z}{3} + \frac{\text{sine of } 5z}{5}$ &c. = correction, which should not be sought when $z = 0$ or any multiple of 180, when $z = 90°$, it becomes sine of $90° + \frac{\text{sine of } 3 \times 90°}{3} + \frac{\text{sine of } 5 \times 90°}{5}$ &c. that is $1 - \frac{1}{3} + \frac{1}{5}$ &c. or its equal $\frac{Q}{2}$ for the

correction, the same as LANDEN finds, this is true whilst z is exclusively between 0 and 180°.

4. In *Cor.* II. *Example* 1. *Theorem* I. p being $= 1$ we have sine of $z +$ sine of $3z +$ sine of $5z$ &c. $= \frac{1}{2 \text{ sine of } z}$ failing (from above) when qz or $2z = 0$, or a multiple of 360°, and therefore when $z = 0$, or a multiple of 180°; if we multiply this by $\dot{z}$ and find the fluent we have, because $\frac{\dot{z}}{2 \text{ sine of } z}$ (by putting y for the sine of z) $= \frac{y^{-1}\dot{y}}{2\sqrt{1-y^2}} = \frac{y^{-2}\dot{y}}{2\sqrt{y^{-2}-1}} = -\frac{\dot{x}}{2\sqrt{x^2-1}}$ (x being put for $\frac{1}{y}$) whose fluent is $= -\frac{1}{2}$ log. of $\overline{x + \sqrt{x^2-1}}$ $= -\frac{1}{2}$ log. of $\frac{1+\sqrt{1-y^2}}{y}$, consequently cos. of $z + \frac{\text{cos. of } 3z}{3} + \frac{\text{cos. of } 5z}{5} = \frac{1}{2}$ log. of $\frac{1+\sqrt{1-y^2}}{y} +$ correction, which being sought when $z = 90°$ and consequently $y = 1$ and the cosines of z, of $3z$, of $5z$, &c. $= 0$, we have it equal to 0; and this has no failing case since it will not fail when $z = 0$ or any multiple of 180° in which primitive equation does. If z be $= 45°$ we shall have cos. of $z = \sqrt{\frac{1}{2}}$, cos. of $3z = -\sqrt{\frac{1}{2}}$, cos. of $5z = -\sqrt{\frac{1}{2}}$, cos. of $7z = +\sqrt{\frac{1}{2}}$, cos. of $9z = +\sqrt{\frac{1}{2}}$, &c. therefore $\sqrt{\frac{1}{2}} \times : 1 - \frac{1}{3} - \frac{1}{5} + \frac{1}{7} + \frac{1}{9} -$ &c. or $\sqrt{\frac{1}{2}} \times 1 - \frac{8}{3.5} + \frac{16}{7.9} - \frac{24}{11.13}$ &c. $= \frac{1}{2}$ log. of $\frac{1+\sqrt{\frac{1}{2}}}{\sqrt{\frac{1}{2}}}$ $\therefore 1 - \frac{8}{3.5} + \frac{16}{7.9}$ &c. $= \sqrt{\frac{1}{2}}$. log. of $\frac{1+\sqrt{\frac{1}{2}}}{\sqrt{\frac{1}{2}}} = \sqrt{\frac{1}{2}}$ log. of $\overline{\sqrt{2}+1}$ $\therefore \frac{1}{3.5} - \frac{2}{7.9} + \frac{3}{11.13}$ &c. $= \frac{1}{8} - \frac{\sqrt{\frac{1}{2}}}{8}$ log. of $\overline{\sqrt{2}+1}$.

5. By *Theorem* I. *Example* 6; we have the sum of the series sine of $p + g$ sine of $\overline{p+q} \cdot z + g^2$ sine of $\overline{p+2q} \cdot z +$ &c.

$= \frac{-g \text{ sine of } \overline{p-q}\,.\,z + \text{sine of } pz}{g^2+1-2g \text{ cos. of } qz}$, if this should have a failing case, it will be, by this scholium, when $qz = 0$ or some multiple of $360°$ provided g be affirmative; but if g be negative, it will be when qz is some odd multiple of $180°$; a similar expression to this is given by Mr. LANDEN by his method.

If p be $= q = 1$ we shall have, sine of $z + g$ sine of $2z + g^2$ sine of $3z$ &c. $= \frac{\text{sine of } z}{g^2+1-2g \text{ cos. of } z}$, if we now multiply by $\dot{z}$ calling the cosine of z, x and find the fluent, we shall have cos. of $z + \frac{g \text{ cos. of } 2z}{2} + \frac{g^2 \text{ cos. of } 3z}{3}$ &c. $= \frac{1}{2g}\,.$ log. of $\overline{1+g^2-2gx}$ which has no failing case.

6. According to this *General Scholium*, *Example* 2. to *Theorem* II. has failing cases in the investigation, unless the series $1, r, r.\frac{r+1}{2}$ &c. converge; thus those in the *Corollaries* I. II. and III. when qz is any odd multiple of $180°$. By bringing both series to one side in the equations in *Cor.* II. we have sine of $\overline{qr-p}\,.\,z +$ sine of $pz - r \times$ sine of $\overline{q.\overline{r+1}-p}\,.\,z +$ sine of $\overline{p+q}\,.\,z$ $+$ &c. $= 0$, and cos. of $\overline{qr-p}\,.\,z -$ cos. of $pz - r \times$ cos. of $\overline{q.\overline{r+1}-p}\,.\,z -$ cos. of $\overline{p+q}\,.\,z +$ &c. $= 0$. Multiply them both by $\dot{z}$ and take the correct fluents when $z = 0$, and we get

$$\frac{\text{cos. of } \overline{qr-p}\,.\,z}{qr-p} + \frac{\text{cos. of } pz}{p} - r\,.\,\frac{\text{cos. of } \overline{q.\overline{r+1}-p}\,.\,z}{q.\overline{r+1}-p} + \frac{\text{cos. of } \overline{p+q}\,.\,z}{p+q} +$$

$$r\,.\,\frac{r+1}{2}\,.\,\frac{\text{cos. of } \overline{q.\overline{r+2}-p}\,.\,z}{q.\overline{r+2}-p} + \frac{\text{cos. of } \overline{p+2q}\,.\,z}{p+2q} \text{ \&c.} = M, \text{ and}$$

$$\frac{\text{sine of } \overline{qr-p}\,.\,z}{qr-p} - \frac{\text{sine of } pz}{p} - r\,.\,\frac{\text{sine of } \overline{q.\overline{r+1}-p}\,.\,z}{q.\overline{r+1}-p} - \frac{\text{sine of } \overline{p+q}\,.\,z}{p+q}$$

$+$ &c. $= 0$, M being put for $\frac{1}{qr-p} + \frac{1}{p} - r\,.\,\frac{1}{q.\overline{r+1}-p} + \frac{1}{p+q}$ $+ r\,.\,\frac{r+1}{2}\,.\,\frac{1}{q.\overline{r+2}-p} + \frac{1}{p+2q}$ &c. $= \frac{qr}{p.\overline{qr-p}} - r\,.\,\frac{\overline{r+2}\,.\,q}{\overline{p+q}\,.\,\overline{q.\overline{r+1}-p}}$

$+ r . \frac{r+1}{2} . \frac{\overline{r+4} . q}{\overline{p+2q} . q . \overline{r+2} - p}$ &c., if we now multiply the first of these by sine of $\overline{\frac{1}{2}qr - p} . z$, and the second by cos. of $\overline{\frac{1}{2}qr - p} . z$, and take the difference we have

$\frac{\text{sine of } \overline{\frac{1}{2}qr - p} . z \times \text{cos. of } \overline{qr - pz} - \text{cos. of } \overline{\frac{1}{2}qr - p} . z \times \text{sine of } \overline{qr - pz}}{qr - p} + [\text{sine of } \overline{\frac{1}{2}qr - p} . z . \text{cos. of } pz + \text{cos. of } \overline{\frac{1}{2}qr - pz} . \text{sine of } pz] \div p - \frac{r}{q . \overline{r+1} - p} \times [\text{sine of } \overline{\frac{1}{2}qr - pz} . \text{cos. of } \overline{\overline{r+1} . q - p} . z - \text{cos. of } \overline{\frac{1}{2}qr - p} . z . \text{sine of } \overline{\overline{r+1} . q - p} . z] - \frac{r}{p+q} \times [\text{sine of } \overline{\frac{1}{2}qr - p} . z . \text{cos. of } \overline{p+q} . z + \text{cos. of } \overline{\frac{1}{2}qr - p} . z \times \text{sine of } \overline{p+q} . z]$ &c. which by trigonometry is reducible to $- \frac{\text{sine of } \frac{1}{2}qr . z}{qr - p} + \frac{\text{sine of } \frac{1}{2}qrz}{p} + r \frac{\text{sine of } q . \overline{\frac{1}{2}r + 1} . z}{q . \overline{r+1} - p} - r \frac{\text{sine of } q . \overline{\frac{1}{2}r + 1} . z}{p+q}$ &c. $= \text{M} . \text{sine of } \overline{\frac{1}{2}qr - pz}$; or $\frac{\text{sine of } \frac{1}{2}qrz}{p . \overline{qr - p}} - r \frac{\text{sine of } q . \overline{\frac{1}{2}r + 1} z}{\overline{p+q} . \overline{q . \overline{r+1} - p}} = \text{M} . \frac{\text{sine of } \overline{\frac{1}{2}qr - p} . z}{qr - 2p}$, the same as LANDEN finds, page 83, Mem. 5. We may farther add, that when series are obtained from others having failing cases by substitution, as in *Scholium* III. to *Theorem* I. or as in *Theorem* V. and VI. regard should be had to those failing cases, according to the manner of substitution, in order to find the failing cases in the new series. We might proceed to many more examples, in finding the sums of new series from others multiplied by fluxions, or we might give examples of finding the sums of new series by throwing others into fluxions: but my chief object in these latter examples was to obviate any difficulty that might appear in choosing the cases for the correction of the fluents. There are other inferences to be drawn, which I may perhaps consider at some future period.

VIII. *An Account of a small Lobe of the human prostate Gland, which has not before been taken Notice of by Anatomists.* By Everard Home, *Esq. F.R.S.*

Read February 20, 1806.

DISCOVERIES in the anatomy of the human body have been ever considered by this learned Society as deserving a place in the Philosophical Transactions: in the present improved state of our knowledge of this subject, a small addition to it cannot fail of being acceptable, since after the long continued labours of so many acute observers, such only can be expected, and even those are rarely to be acquired.

The subject of the present Paper is a portion of a gland, which from the obscurity of its situation has hitherto escaped observation: and were it not for the change produced in it by disease, which enlarges it so much that it sometimes completely shuts up the canal, by which the urine ought to pass, it would be little deserving of attention: but when this important effect is considered, the part itself becomes an object of very serious interest.

In stating the circumstances, which led to the present investigation, it may be necessary to mention that the prostate gland is liable in the latter period of life to enlarge: and when it does so there is frequently a nipple-like projection, which rises up and forms tumours of very different sizes in

the cavity of the bladder. These tumours, as they obstruct the passage of the urine, have attracted the attention of all anatomical surgeons, from the time of MORGAGNI to the present day. Their appearance has been accurately described, and specimens of them in different degrees of enlargement are preserved in every collection of morbid parts. The attention of surgeons has been naturally called to what is of the greatest consideration, the appearances they put on, and the symptoms they produce: but the particular circumstances in the natural conformation of the gland, which dispose it to form these tumours, have never been examined. MORGAGNI says, "These caruncles were found to grow out in the very "middle of the upper and internal posterior circumference of "the gland; but whether these things happened by chance or "otherwise future observations will shew."*

From these expressions, it is evident that MORGAGNI had no idea that there was any conformation of the prostate gland, that could account for this tumour, and believed that it arose from the surface of the body of the gland.

Mr. HUNTER, in treating of the enlargement of the prostate gland, says, "From the situation of the gland, which is prin- "cipally on the two sides of the canal, and but little if at all "on the fore part, as also very little on the posterior side, "when it swells it can only be laterally; whereby it presses

* Si ea, quæ ex Sepulchreto exempla indicavimus, et id, quod supra ex Valsalva attulimus, et nostra omnia attentè inspicias, cuncta in senibus fuisse animadvertes: ita nostra omnia, in quibus carunculæ initium fuit, hanc in medio ipso posteriori interni summique glandulæ ambitûs excrescentem obtulisse: casune hæc cuncta, an secus, futuræ ostendent observationes. MORGAGNI *de Sed. et Caus. Morb.* lib. iii. epist. 41. A. 19.

"the two sides of the canal together, and at the same time "stretches it from the anterior edge or side to the posterior, "so that the canal instead of being round, is flattened into a "narrow groove. Sometimes the gland swells more on one "side than the other, which makes an obliquity in the canal "passing through it.

"Besides this effect of the lateral parts swelling, a small "portion of it, which lies behind the very beginning of the "urethra swells forward like a point, as it were, into the "bladder; acting like a valve to the mouth of the urethra, "which can be seen, even when the swelling is not consi-"derable, by looking on the mouth of the urethra, from the "cavity of the bladder, in the dead body. It sometimes in-"creases so much, as to form a tumour projecting into the "cavity of the bladder some inches."*

From the first paragraph it is evident that Mr. HUNTER was unacquainted with this lobe; and in the second we see that his knowledge of the disease led him to conclude, that in the natural state of the gland there was a portion of it in this situation: but neither at that time, nor at any future period of his life, did he prosecute the inquiry.

Although a great part of my time has been for many years occupied in attending patients labouring under complaints of the bladder and urethra, and my opportunities of examining these parts after death have been very frequent, my attention has been always so much employed on the modes of emptying the bladder, (an operation, which in many cases is attended with considerable difficulty,) that it never occurred to me to institute an inquiry for the purpose of attaining an accurate

* HUNTER on the Venereal Disease, page 169.

knowledge of the extent of the disease until the month of December, 1805.

At that time my attention was directed to this subject by the following circumstances. In the examination of the prostate gland of an elderly person, who had died in consequence of this part having been diseased, the nipple-like process was found very prominent, and a bridle, nearly ¼ of an inch in breadth, extended from the middle line of the tumour to the bulb of the urethra, where it insensibly disappeared. The usual rounded projection of the *caput gallinaginis* was not visible: it had wasted away, and the remains were concealed in the fold forming this bridle, which at that part was not thicker than at any other. The space between the tumour in the bladder, and the bulb of the urethra was unusually short, which is the reverse of what is commonly met with in old men; so that this bridle appeared to have drawn the bulb towards the tumour, and shortened the membranous part of the canal.

As this was an unusual appearance, it led me to consider it with attention, and to ask if other anatomists had noticed it; which as far as my inquiries have gone has not been the case. The bridle had evidently been formed by the membrane of the bladder adhering firmly to that part of the prostate gland composing the tumour, which it consequently followed in its future increase, and drew up after it the membrane of the urethra. In this way the fold had in time become nearly ¼ of an inch broad, and was continued of the same breadth to the bulb, where the lining of the urethra being more attached to the surrounding parts, it did not admit of being drawn up.

This appearance of a bridle is more or less met with in all the cases, in which the nipple-formed process occurs, but in so much smaller a degree, and not continued beyond the caput gallinaginis, that it never before led me to pay attention to it.

To satisfy myself how this tumour was formed, it became necessary to examine the prostate gland in its natural state: and ascertain whether there is any part sufficiently detached to move independent of the rest of the gland, and explain the appearances which had been met with in this particular case.

My professional avocations not affording time to make the dissections requisite for this purpose, Mr. Brodie, Demonstrator of Anatomy to Mr. Wilson, Teacher of Anatomy, in Windmill-street, whose knowledge of the subject fitted him for the task, and whose zeal for the improvement of his profession made him willingly undertake it, gave me his assistance, and took the whole of that labour on himself.

While dissecting the parts for this purpose, the urinary bladder was distended with water, and the surfaces of the prostate gland, vesiculæ seminales, and vasa deferentia, were fairly exposed. This being done, the vasa deferentia, and vesiculæ seminales were carefully dissected off from the bladder, without removing any other part. These were turned down upon the body of the prostate gland. An accurate dissection was then made of the circumference of the two posterior portions of the prostate gland, and the space between them was particularly examined. In doing this a small rounded substance was discovered, so much detached that it seemed a distinct gland, and so nearly resembling Cowper's glands in size and shape, as they appeared in the same subject, in which they were unusually large, that it appeared to be a gland of

that kind. It could not however be satisfactorily separated from the prostate gland, nor could any distinct duct be found leading into the bladder.

A similar examination was made of this part in five different subjects. The appearance was not exactly the same in any two of them. In one there was no apparent glandular substance, but a mass of condensed cellular membrane: this, however, on being cut into, differed from the surrounding fat. In another there was a lobe blended laterally with the sides of the prostate gland. These facts are mentioned in proof of its not being always of the same size nor having exactly the same appearance; this is found also to be the case with COWPER's glands, they are sometimes large and distinct; in other subjects are scarcely to be detected, and in others again are in all the intermediate states. The most distinct and natural appearance of this part was in a healthy subject 25 years of age, of which the following is an account. On turning off the vasa deferentia and vesiculæ seminales, exactly in the middle of the sulcus, between the two posterior portions of the prostate gland, there was a rounded prominent body, the base of which adhered to the coats of the bladder. It was imbedded not only between the vasa deferentia and the bladder, but also in some measure between the lateral portions of the prostate gland and the bladder, since they were in part spread over it, so as to prevent its circumference from being seen, and they adhered so closely as to require dissection to remove them; nor could this be done beyond a certain extent, after which the same substance was continued from the one to the other. This proved it to be a lobe of the prostate gland, the middle of which had a rounded form, united to the gland at the base

next the bladder, but rendered a separate lobe by two fissures on its opposite surface. Its ducts passed directly through the coats of the bladder, on which it lay, and opened immediately behind the verumontanum. By means of this lobe a circular aperture is formed in the prostate gland, which gives passage to the vasa deferentia.

The appearance of this lobe has been since examined in a subject 24 years of age, and it was found still larger and more distinct. A representation of it is given in the annexed Plate.

Previous to this investigation it was not known to me that any distinct portion of the prostate gland was situated between the vasa deferentia and the bladder. These ducts were considered to pass in the sulcus between its two posterior portions, in close contact with the body of the gland. This account corresponds also with the description given by WINSLOW and HALLER; it is however now proved to be erroneous. It is not in my power to determine whether all the anatomists of the present day have fallen into this error in the same degree with myself: but none of them have pointed out this lobe; and, therefore, in whatever way they have described the vasa deferentia to pass into the bladder, they have neither anticipated nor thrown any light on the present inquiry.* HALLER says expressly, that "the prostate gland has no lobular appearance," and the anxiety which all anatomists have to improve their art, would have led them to correct this error, had they discerned that it was one.

This newly acquired anatomical fact enables us very clearly

* Glandula, aut certe cellulosum compactum corpus, quod prostata dicitur. P. 464. Fabrica obscura est, et neque glandulæ simplicis similis, cujus cavea esset aliqua, neque compositæ; neque enim in lobulos recte discedit. P. 465. *Elem. Physiologiæ Corporis Humani, Autore* ALBERT. HALLER. Tom. VII.

to understand the nature of a disease, which it was not possible we could have a correct idea of, when we were ignorant of the existence of the part in which it takes place. It not only explains the situation of the tumour, the want of connection with the body of the gland, and the narrowness of its base where that is met with, but it solves what has ever appeared to me the greatest difficulty, how it should protrude into the cavity of the bladder. This arises from the hard substance of the coats of the vasa deferentia being in close contact, and bound down upon this lobe, so that from its first enlargement it must immediately press up the inner membrane of the bladder, which can make very little resistance.

This lobe of the prostate gland, from its situation and connection with the vasa deferentia, is liable to many causes of swelling, which the body of the gland itself is free from; for every irritation upon the seminal vessels or their orifices may be communicated to it by continuity of parts: and aged men, from an ignorance of these facts, are too often, through imprudence, producing an excitement in those vessels which the parts are unable to support; and when this is long continued, inflammation becomes the consequence, which cannot take place to any degree without being communicated to this lobe, and producing an enlargement of it.

Every violent effort which is made to empty the urinary bladder produces an unusual pressure against this lobe, by which it may be injured. There is also much reason for believing, that the diseased state of the lateral parts of the gland, so very commonly met with in the latter period of life, has its origin in this particular lobe; since in most of the cases of a diseased state of the gland, which have come under my observation after death, this lobe has been enlarged in a

greater degree in proportion to its size than any other part; and in some instances the enlargement of it has been very great, while it appeared to be only beginning in the lateral portions.

Difficulty in passing the urine is a symptom, which comes on very early in diseases of the prostate gland, and arises entirely from this lobe being increased in size, since any enlargement in the lateral portions of the gland widens the canal instead of diminishing it, and they do not require much force to separate them; but the least increase of this lobe tends to shut it up.

The enlargement of this lobe produces an effect which is not generally known, and leads medical practitioners into an error respecting the nature of the complaint. The orifice of the urinary bladder, which is the lowest part in the natural state, is raised up in proportion to the increase of this lobe; so that none of the contents below that level can be expelled, although whatever is above it is allowed with more or less difficulty to pass out. In this way the person never evacuates more than one half or one third of the urine contained in the bladder; but as the water which comes away passes in a stream, and the quantity voided in 24 hours is sufficient, no suspicion is entertained of the cause of the frequency and distress in passing it, and the symptoms are referred to an irritable state of the coats of the bladder. It is only by drawing off the urine through a catheter that the disease in this lobe can be ascertained; as in that way alone the quantity of urine which is retained can be determined. No examination *per anum* can give the surgeon any information on this subject; since the posterior surface of the vasa deferentia only is to be felt, if the finger should reach so far; and yet it is in this way that practitioners in general pretend to judge of the greater or

less degree of the disease, although that portion of the gland, which forms the most important part of the complaint is wholly out of their reach

The least projection of this lobe into the bladder stretches the internal membrane of that viscus which passes over it, keeps it in a state of irritation, and makes it liable to be grasped by the action of the sphincter muscle in expelling the last drops of urine, so as to give the patient excruciating pain. When it is more enlarged these symptoms go off.

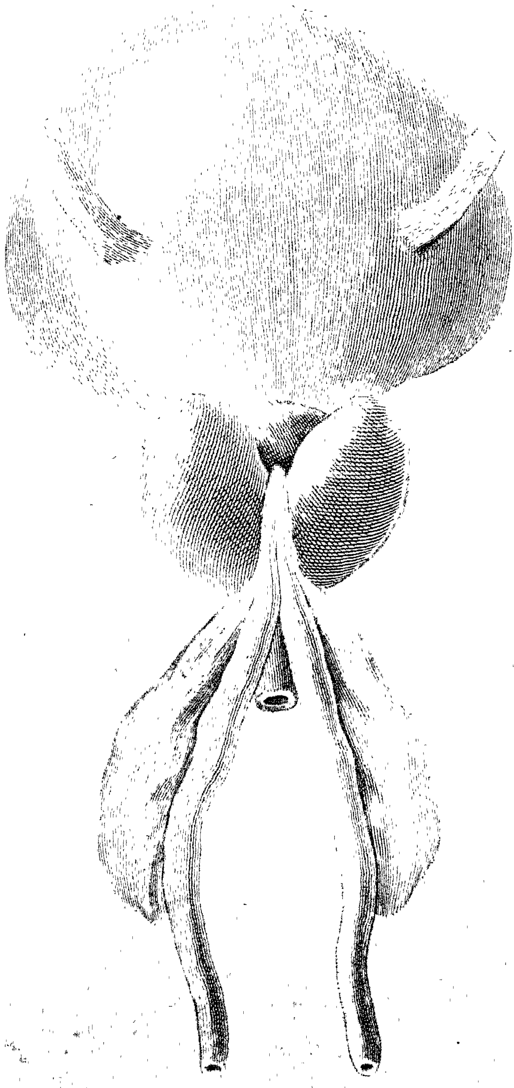
From these observations it appears, that this small lobe of the prostate gland, which has been overlooked, is from the situation and the circumstances, in which it is placed more liable to become diseased than any other part of the gland, and produces symptoms of danger and distress peculiar to itself, which have been hitherto supposed to arise from the body of the gland becoming enlarged.

To enter further into the effects of disease on this lobe would be improper on the present occasion; but not to have noticed them at all would have been equally so, since the only importance, that can be attached to the facts, which have been brought forward in this Paper, arises from the light they throw on the diseases of the prostate gland.

EXPLANATION OF THE PLATE. (Plate III.)

A posterior view of the outside of the bladder and prostate gland.

The vesiculæ seminales and vasa deferentia are dissected off and turned forwards, to shew the newly discovered lobe, which lies between them and the bladder. The two posterior parts of the lateral portions are spread open to expose the lobe lying between them.



IX. *On the Quantity and Velocity of the Solar Motion.* By William Herschel, *LL. D F R. S.*

Read February 27, 1806.

THE direction of the solar motion having been sufficiently ascertained in the first part of this Paper,* we shall now resume the subject, and proceed to an inquiry about its velocity.

The proper motions, when reduced to one direction, have been called quantities, to distinguish them from the velocities required in the moving stars to produce those motions. It will be necessary to keep up the same distinction with respect to the velocity of the solar motion; for till we are better acquainted with the parallax of the earth's orbit, we can only come to a knowledge of the extent of the arch which this motion would be seen to describe in a given time, when seen from a star of the first magnitude placed at right angles to the motion. There is, however, a considerable difference between the velocity of the solar motion and that of a star; for at a given distance, when the quantity of the solar motion is known its velocity will also be known, and every approximation towards a knowledge of the distance of a star of the first magnitude will be an approximation towards the knowledge of the real solar velocity; but with a star it will be otherwise; for though the situation of the plane in which it moves is

* Phil. Trans. for 1805, page 231.

given, the angle of the direction of its motion with the visual ray will still remain unknown.

As hitherto we have consulted only those proper motions which have a marked tendency to a parallactic centre, we ought now, when the question is to determine the velocity of the solar motion, to have in view the real motion of every star whose apparent motion we know; for as it would not be proper to assign a motion to the sun, either much greater or much less than any real motion which may be found to exist in some star or other, it follows that a general review of proper motions ought to be made before we can impartially fix on the solar velocity; but as trials with a number of stars would be attended with considerable inconvenience, I shall use only our former six in laying down the method that will be followed with all the rest.

Proportional Distance of the Stars.

We are now come to a point no less difficult than essential to be determined. Neither the parallactic nor real motion of a star can be ascertained till its relative distance is fixed upon. In attempting to do this it will not be satisfactory to divide the stars into a few magnitudes, and suppose *these* to represent the relative distances we require. There are not perhaps among all the stars of the heavens any two that are exactly at the same distance from us; much less can we admit that the stars which we call of the first magnitude are equally distant from the sun. And indeed, if the brightness of the stars is admitted as a criterion by which we are to arrange them, it is perfectly evident that all those of the first magnitude must differ as much in distance as they certainly do in

lustre; yet imperfect as this may be, it is at present the only rule we have to go by.

The relative brightness of our six stars, may be expressed as follows: Sirius --- Arcturus - Capella -, Lyra -- Aldebaran . Procyon.

The notations here used are those which have been explained in my first Catalogue of the relative Brightness of the Stars;* but to denominate the magnitudes of these six stars so that they may with some probability represent the distances at which we should place them according to their relative brightness, I must introduce a more minute subdivision than has been commonly admitted, by using fractional distinctions, and propose the following arrangement.

Table VIII.

Proportional Distances of Stars.

Sirius - -	1,00	Lyra - -	1,30
Arcturus -	1,20	Aldebaran -	1,40
Capella - -	1,25	Procyon - -	1,40

The interval between Sirius and Arcturus is here made very considerable; but whoever will attentively compare together the lustre of these two stars, when they are at an equal altitude must allow that the difference in their brightness is fully sufficient to justify the above arrangement.

The order of the other four stars is partly a consequence of the distance at which Arcturus is placed, and of the comparative lustre of these stars such as it has been estimated by observations. But if it should hereafter appear that other

* Phil. Trans. for 1796, page 189.

more exact estimations ought to be subtituted for them, the method I have pursued will equally stand good with such alterations. I have tried all the known, and many new ways of measuring the comparative light of the stars, and though I have not yet found one that will give a satisfactory result, it may still be possible to discover some method of mensuration preferable to the foregoing estimations, which are only the result of repeated and accurate comparisons by the eye. Whenever we are furnished with more authentic data the calculations may then be repeated with improved accuracy.

Effect of the Increase and Decrease of the Solar Motion, and Conditions to be observed in the Investigation of its Quantity.

The following Table, in which the 2d, 4th, and 5th columns contain the sides of the parallactic triangle, is calculated with a view to show that an increase or decrease of the solar motion will have a contrary effect upon the required real motions of different stars; and as we are to regulate the solar velocity by these real motions, an attention to this circumstance will point out the stars which are to be selected for our purpose

Table IX.

Stars and relative Distances.	Apparent Motion.	Solar Motion.	Parallactic Motion.	Real Motion.	Velocities.
Sirius 1,00	1″,11528	1,0 1,5 2,0	0″,67768 1,01652 1,35536	+ 0″,46518 + 0,21701 — 0,32776	465175 217007 327755
Arcturus 1,20	2″,08718	1,0 1,5 2,0	0,53579 0,80368 1,07158	+ 1,57389 + 1,30478 + 1,01561	1888670 1565735 1218736
Capella 1,25	0″,46374	1,0 1,5 2,0	0,79593 1,19390 1,59186	— 0,42159 — 0,79637 — 1,18662	526987 995465 1483270
Lyra 1,30	0″,32435	1,0 1,5 2,0	0,32542 0,48812 0,65083	— 0,47065 — 0,59923 — 0,74135	611839 778995 963750
Aldebaran 1,40	0″,12341	1,0 1,5 2,0	0,65117 0,97676 1,30234	— 0,53208 — 0,85737 — 1,18283	744913 1200324 1655967
Procyon 1,40	1″,23941	1,0 1,5 2,0	0,66394 0,99591 1,32788	+ 0,59548 + 0,30731 — 0,23385	833665 430227 327390

The real motion of Arcturus contained in the 5th column compared with that of Aldebaran, shows that when the solar motion is increased from 1,0 to 1,5 and to 2″,0 the real motion of Arcturus will be gradually diminished from 1,57 to 1,30 and to 1″,02, while that of Aldebaran undergoes a con-

trary change from 0,53 to 0,86 and to 1″,18. We may also notice that Capella and Aldebaran, which have a negative sign prefixed to their real motions when the solar motion is 1″,0 are affected differently from Arcturus, Sirius, and Procyon, which have a positive sign; and that even the motions of the two last become negative when the solar motion is increased beyond a certain point. It may be easily understood that the motion of Arcturus itself would become negative were we to increase the solar motion till the parallactic motion of this star should exceed its apparent motion.

From these considerations it appears, that a certain equalization, or approach to equality may be obtained between the motions of the stars, or between that of the sun and any one of them selected for the purpose; for instance, the motions of Arcturus and Aldebaran being contrary to each other, may be made perfectly equal by supposing the sun's annual motion to be 1″,85925. For then we shall have the real annual motion of Arcturus towards the parallactic centre 1″,091, and that of Aldebaran towards the opposite part of the heavens, in which the solar apex is placed, will be 1″,091 likewise; the first in a direction 55° 29′ 39″ south-preceding, the latter 88° 16′ 31″ north-following their respective parallels; and a composition of these motions with the parallactic ones arising from the given solar motion, will produce the apparent motions of these stars which have been established by observation. But since Arcturus, by the hypothesis which has been adopted in Table VIII. is a nearer star than Aldebaran, the velocities of the real motions, describing these equal arches will be 1309109 in the former and 1527780 in the latter. And it is not the arches but these velocities that

must be equalized. Therefore, in order to have this required equality, let the solar motion be $1'',718865$ then will a velocity of 1399478 in Arcturus, and 1399842 in Aldebaran, which are sufficiently equal, occasion such angular real motions in the two stars as will bring them, when compounded with their parallactic motions, to the apparent places in which we find them by observation.

Before we proceed, it will be proper to obviate a remark that may be made against this way of equalization or approach to equality. We have said that the calculated velocities are such as would be true if the stars were at the assumed distances, and if their real motions were performed in lines at right angles to the visual ray; to which it may here be objected that the last of these assumptions is so far from having any proof in its favour that even the highest probability is against it. We may admit the truth of what the objection states, without apprehending that any error could arise on that account, if the solar motion were determined by this method. For if the stars do not move at right angles to the visual ray, their real velocity will exceed the calculated one; so that in the first place we should certainly have the minimum of their velocities: and if we were obliged, for want of data to leave the other limit of the motion unascertained, it must be allowed to be a considerable point gained if we could show what is likely to be the least velocity of the solar motion; but a more satisfactory defence of the method is, that if we were to assume a mean of all the angular deviations from the perpendicular to the visual ray that may take place in the directions of the real motions of the stars, the only position we could fix upon as a mean would be an

inclination of 45 degrees. For in this case the chance of a greater or smaller deviation would be equal; and when a number of stars are taken, the deviations either way might then be supposed to compensate each other; but what is chiefly to our purpose, not only the angle of 45 degrees, but also any other, that might be fixed upon as a proper one to represent the mean quantity of sidereal motions, would lead exactly to the same result of the solar velocity to be investigated. For if the velocities of any two stars were equalized, when their motions are supposed to be perpendicular to the visual ray, they would be as much so when they make any other given angle with it; and it is the equalization or approach to equality and not the quantity of the velocities that is the spirit of this method. I have only to add, that an equalization of the solar motion with that of any star selected for the purpose may be had by a direct method of calculation, and will therefore be of great use in settling the rate of the motion to be determined.

It must be evident from what has been said, that a certain mean rate, or middle rank, should be assigned to the motion of the sun, unless very sufficient reasons should induce us to depart from this condition. To obtain this end must consequently be our principal aim; and if we can at the same time bring the sidereal motions to a greater equality among each other, it will certainly be a very proper secondary consideration.

There are two ways of taking a mean of the sidereal motions, one of them may be called the rate and the other the rank. For instance, a number equal to the mean rate of the six numbers, 2, 6, 13, 15, 17, 19, would be 12; but one

that should hold a middle rank between the three highest and three lowest of the six would be 14. In assigning the rate of the solar motion it appears to be most eligible that it should hold a middle rank among the sidereal velocities. We shall however find that nearly the same result will be obtained from either of the methods.

With respect to our second consideration, we may see that it also admits of a certain modification by the choice of the solar motion; for in Table IX. when this motion is 1″,5 the velocity of Arcturus 1565735, will exceed that of Sirius, 217007, more than seven times; whereas a solar motion of 1″ will give us the proportional velocities of these stars as 188867 to 465174; and the former will then exceed the latter only four times.

Calculations for drawing Figures that will represent the observed Motions of the Stars.

The necessary calculations for investigating the solar motion are of considerable extent, and may be divided into two classes, the first of which will remain unaltered whatsoever be the solar motion under examination, while the other must be adjusted to every change that may be required.

The direction of the sun remaining as it has been settled in the first part of this Paper, the permanent computation of each star will contain the annual quantity of the observed or apparent motion, its direction with the parallel of the star, its direction with the parallactic motion, and its velocity. The changeable part will consist of the angular quantity of the real motion, the parallactic direction of this motion, and its velocity.

Before we can make a calculation of the required velocities,

we must fix upon the probable relative distance of the rest of the stars, in the same manner as we have done with the first six. In this I have thought it advisable to distinguish the stars that, from their lustre, may be called principal, and have limited their extent to the brightest of the second magnitude, on account of the uncertainty which still remains about their progressive distances. For though it appears reasonable to allow that the bright stars of the second magnitude may be twice as far from us as those of the first, it will admit of some doubt whether this rule ought to be strictly followed up to the 3d, 4th, 5th, and 6th magnitude; especially when it is not easy to ascertain the boundaries which should limit the magnitudes of very small stars.

The number of these principal stars is 24. The remaining 12 are also arranged by admitting that their magnitudes express their relative distances; and notwithstanding the doubtfulness we have noticed, their testimony with respect to the proper quantity of a solar motion, though it should be received with some diffidence, must not be neglected; some considerable alteration in their supposed distances, however, would have but little effect upon the conclusions intended to be drawn from their velocities.

The following Table contains the result of the calculations that relate to the permanent quantities. In the first and second columns, we have the names of the stars, and their assigned relative distances. The third gives the apparent angular motions, and the fourth their direction. The fifth contains the direction of the same motions, with respect to the parallactic motions arising from the given solar direction; and the sixth gives the velocity of the stars which produce the quantity of the apparent motions.

Table X.

Names of the Stars.	Proportional Distances.	Apparent Motions.	Direction with the Parallel.	Direction with the parallactic Motion.	Velocity of the Stars.
Sirius -	1,00	″ 1,11528	68.49.40,7 *sp*	10.24.44,3 *sf*	1115281
Arcturus	1,20	2,08718	55.29.42,0 *sp*	0. 0. 3 *sp*	2504621
Capella -	1,25	0,46374	71.35.22,4 *sf*	24.40.21 *sf*	579668
Lyra -	1,30	0,32435	56.20.57,3 *nf*	92.49.30 *nf*	421657
Rigel -	1,35	0,16273	79.29.33,9 *np*	159.28. 1 *np*	219684
α Orionis	1,35	0,13038	85.38.14,6 *nf*	169.18.58 *np*	176010
Procyon	1,40	1,23941	50. 2.24,5 *sp*	9.40.46 *sp*	1735172
Aldebaran	1,40	0,12341	76.29.37,3 *sf*	13.41.48 *sf*	172778
Pollux -	1,42	0,65037	0. 0. 0 *prec.*	61.30.34 *sp*	923523
Spica -	1,44	0,19102	84. 5. 1,8 *np*	144.13.16 *np*	275065
Antares -	1,46	0,26000	90. 0. 0 *north*	178.57.44 *np*	379600
Altair -	1,47	0,71912	48.40.12,0 *nf*	103.17.29 *nf*	1057105
Regulus -	1,48	0,22886	20.27.37,5 *np*	70. 9.20 *sp*	338711
β Leonis	1,50	0,55324	7.16. 8,4 *sp*	40.34.31 *sp*	829856
β Tauri -	1,50	0,10039	84.58.27,1 *sf*	13.17.11 *sf*	150579
Fomalhaut	1,50	0,30698	11,16.16,3 *nf*	16.47. 5 *sf*	460469
α Cygni	1,60	0,06440	27.45.56,3 *np*	177.31.39 *np*	103036
Castor -	2,00	0,13294	17.30.40,6 *sp*	45.25.43 *sp*	265869
α Ophiuchi	2,00	0,07698	40.30.24,8 *sf*	33.29.28 *sf*	153955
α Coronæ	2,00	0,23279	7.24.15,4 *sf*	105. 0.43 *nf*	465587
α Aquarii	2,00	0,20615	67.10.17,1 *np*	162.43.46 *nf*	412295
α Andromedæ	2,00	0,09268	40.20.48,2 *sf*	12.55.11 *sf*	185360
α Serpentis	2,00	0,21913	60. 7.12,5 *nf*	161.34. 4 *nf*	438257
α Pegasi	2,00	0,18917	72. 5.16,0 *np*	157.45.25 *nf*	378338
α Hydræ	2,30	0,16598	57.30.24,8 *np*	107. 6.24 *np*	381763
α^2 Libræ -	2,40	0,18376	54.42.52,9 *np*	127. 3. 7 *np*	441022
γ Pegasi	2,50	0,17355	59.48. 7,9 *np*	174. 5.15 *nf*	433880
α Arietis -	2,50	0,11587	37. 9.15,9 *sf*	29.32.47 *sf*	289685
α Ceti -	2,80	0,14406	33.44. 2,9 *np*	141.18.55 *np*	403356
α Herculis	3,00	0,23000	90. 0. 0 *north*	168.23.41 *nf*	690000
β Virginis	3,00	0,77706	17.59.25,5 *sf*	111.11.44 *nf*	2331169
γ Aquilæ	3,00	0,19320	55.54.41,7 *np*	178.25.20 *nf*	579589
α^2 Capricorni	3,50	0,26452	79.23.35,3 *nf*	136.21.18 *nf*	925819
β Aquilæ	4,00	0,35127	85. 7.37,0 *sp*	39.49.15 *sp*	1405079
α^1 Capricorni	4,20	0,28000	90. 0. 0 *north*	146.59.44 *nf*	1176000
α^1 Libræ	6,00	0,20898	59.27.58,4 *np*	131.46. 7 *np*	1253875

The contents of this Table will enable us to examine the motions of the stars in different points of view. For instance, by the apparent motions in the third column, and their directions in the fourth, a figure may be drawn which will represent the actual state of the heavens, with respect to those annual changes in the situations of our 36 stars, which in astronomical tables are called their proper motions.

Fig. 1, Plate IV. gives us these motions brought into one view, so that by supposing successively every one of the stars to be represented by the central point of the figure, we may see the angular quantity and direction of the several annual proper motions represented by the line which is drawn from the centre to each star. By this means we have the comparative arrangement and quantity of these movements with respect to their directions.

Fig. 3 represents the same motions, but instead of being drawn so as to show their directions with regard to the several meridians and parallels of the stars, they are laid down by the angles contained in the fifth column; and will therefore indicate their arrangement with respect to a line drawn from the solar apex towards the parallactic centre. These directions will remain the same, whatever may be the velocity of the solar motion upon which we shall ultimately fix, provided no change be made in the situation of the apex towards which the sun has been admitted to move.

In these two figures, the lines drawn from the centre give us only the angular changes of the places that have been either observed or calculated, and not the velocities which are required in the stars to produce them. It will therefore be necessary to represent the velocities by two other figures, in

which the same directions are preserved, but where the extent of each line is made proportional to the distance of the stars in the second column.

Fig. 2 is drawn according to this plan; the angles of the directions remain as in the fourth column, but the lines are lengthened so as to give us the velocities contained in the sixth.

In Fig. 4, the angles of the 3d figure are preserved, but the lines are again lengthened as in Fig. 2.

N. B. These two last figures would have been of an inconvenient size if they had been drawn on the same scale with the two foregoing ones, for which reason, in comparing the 2d and 4th with the 1st and 3d, it must be remembered that the former are reduced to one half of the dimensions of the latter.

Remarks on the sidereal Motions as they are represented from Observation.

As we have now before us a set of figures which give a complete view of the result of the calculations contained in the Xth Table, we may examine the arrangements of the stars, and draw a few conclusions from them, that will throw some light upon the subject of our present inquiry.

In the first place, then, we have to observe in Fig. 1, that 17 out of the 21 stars, whose motions are directed towards the north, are crowded together into a compass of little more than $76\frac{1}{2}$ degrees. But this figure, as we have shown, is drawn from observation. We are consequently obliged to conclude, that, if these motions are the real ones, there must be some physical cause which gives a bias to the directions in

which the stars are moving; if so, it would not be improbable that the sun, being situated among this group of stars, should partake of a motion towards the same part of the heavens.

Our next remark concerns the velocity of the sidereal motions; and therefore we must have recourse to Fig. 2, where we perceive that the greatest motions are not confined to the brightest stars. For instance, the velocity of β Virginis is but little inferior to that of Arcturus, and exceeds the velocity of Procyon. Likewise the velocities of β Aquilæ, α′ Libræ, and α′ Capricorni, surpass that of Sirius; and an inspection of the rest of the figure will be sufficient to show how very far the velocities of Capella, Lyra, Rigel, α Orionis, Aldebaran, and Spica, are exceeded by those of many other stars.

If we look at the arrangement of the stars with respect to the direction of the solar motion, we find in Fig. 3, that a somewhat different scattering of them has taken place; but still most of the stars appear to be affected by some cause which tends to lead them to the same part of the heavens, towards which the sun is moving; and the directions of the greatest number of them are not very distant from the line of the solar motion.

The whole appearance of this figure presents us with the idea of a great compression above the centre, arising from some general cause, and a still greater expansion in the lower part of it. The considerable projection of a few stars on both sides, is however a plain indication that the compressing or dilating cause does not act in their directions.

When the velocity of the stars, represented in the same point of view in Fig. 4, is examined, we find a particularity

in the direction and comparative velocities in the largest stars that must not be overlooked. Four of them, Rigel, α Orionis, Spica, and Antares, have a motion towards that part of the heavens in which the solar apex is placed, and their motions are very slow. Three other stars of the 1st magnitude, Arcturus, Procyon, and Sirius, move towards the opposite part of the heavens, and their motions, on the contrary, are very quick.

The direction of the motion of Aldebaran, compared with its small velocity, is no less remarkable; and seems to be contrary to what has been pointed out with the three last mentioned stars; we shall however soon have an opportunity of showing that it is perfectly consistent with the principles of the solar motion.

The Solar Motion and its Direction assigned in the first Part of this Paper are confirmed by the Phenomena attending the observed Motions of the 36 Stars.

An application of some of the foregoing remarks will be our next subject; and I believe it will be found, that in the first place they point out the expediency of a solar motion. That next to this they also direct us to the situation of the apex of this motion: and lastly, that they will assist us in finding out the quantity requisite for giving us the most satisfactory explanation of the phenomena of the observed proper motions of the stars.

In examining the second figure, it has been shown that no less than six stars of the first magnitude, namely, Capella, Lyra, Rigel, α Orionis, Aldebaran, and Spica, have less velocity than nine or ten much smaller stars. Aldebaran and

α Orionis indeed have so little motion that there are but three stars in all the 36 that have less. But the situation of these bright stars, from their nearness, must be favourable to our perceiving their real motions if they had any, unless they were counteracted by some general cause that might render them less conspicuous. Now to suppose that the largest stars should really have the smallest motions, is too singular an opinion to be maintained; it follows, therefore, that the apparently small motions of these large stars is owing to some general cause, which renders at least some part of their real motion invisible to us. But when a solar motion is introduced, the parallax arising from that cause will completely account for the singularity of these slow motions.

If the foregoing argument proves the expediency of a solar motion, its direction is no less evidently pointed out by it. For if the parallax occasioned by the motion of the sun is to explain the appearances that have been remarked, it will follow, that a direction in opposition to the motion of Arcturus, will answer that end in the most satisfactory manner. That compression, for instance, which has been remarked in the motions of the stars moving toward the solar apex in Fig. 3, and which is so completely accounted for by a parallactic motion arising from the motion of the sun, points out the direction in which the sun should move, in order to produce this required parallactic motion. The expansion of the motions that are in opposition to the former is evidently owing to the same parallactic motions, which in this direction unite with the real motions of the stars; and as, in the former case, the observed motions are the differences between the parallactic and real motions, so here they are the sum of them.

The remark that stars having a side motion, are not affected by the cause of the compression or expansion, which acts upon the rest, is perfectly explained; for a parallactic motion, in the direction of the motion of Arcturus, can have no effect in lengthening or shortening the perpendicular distance to which a star may move in a side direction.

I have only to add, that the small velocities of Rigel, α Orionis, Spica, and Antares, in Fig. 4, as well as the great velocities of Arcturus, Procyon, and Sirius, point out the same apex which in the first part of this Paper has already been established by more extended computations.

The case of Aldebaran, though seemingly contrary to what has been shown, confirms the same conclusions. This will appear by considering that a star, moving towards the solar apex with a greater real motion than its parallactic one, must continue apparently to move in its real direction; but should a star, such as Aldebaran, move towards the apex with less velocity than the parallactic motion which opposes it, there will arise a change of direction, and the star will be seen moving towards the opposite part of the heavens.

Trial of the Method to obtain the Quantity of the Solar Motion by its Rank among the sidereal Velocities.

According to the conditions that have been explained, a calculation may be made with a view of equalizing the velocities of the sun and the star α Orionis; and the result of it will show that the proposed equality will be obtained when the solar motion is 1",266230. It will moreover be found than so small an increase of this motion as 0",01 would give us 19 stars with less, and 17 with more velocity than that

which the calculation assigns to the sun; this consequently fixes one of the limits to which the solar motion ought not to come up, if we intend it should hold a middle rank among the sidereal velocities.

On the other hand, by a similar calculation of the velocities of the star Pollux and the sun, it appears that a solar motion of 0″,967754 will make them equal; and that a diminution of this motion not exceeding 0″,01 would give us 19 stars moving at a greater rate than the sun, and only 17 falling short of its velocity. This consequently fixes the other limit to which the solar motion ought not to be depressed. And thus it appears by this method, that the quantity we are desirous of ascertaining, is confined within very narrow bounds, and that by fixing upon a mean of the two limits, we may have the rank of the solar motion true to less than 0″,15.

Calculations for investigating the Consequences arising from any proposed Quantity of Solar Motion, and for delineating them by proper Figures.

Before we can justly examine the real motions of stars which it will be necessary to admit in consequence of a given solar motion, it will be convenient to have them represented in two figures that we may see their arrangement and extent; and as a calculation of the required particulars will oblige us to fix upon a certain quantity, we shall take the motion that has been ascertained to belong to the middle rank of the sidereal velocities for a pattern. The result of the necessary calculations is as follows.

Table XI.

Names.	Parallactic Motion.	Real Motion.	Parallactic Angle.	Velocity.
	″	″	° ′ ″	
Sun - -	0,00000	1,116992	00.00.00	1116992
Sirius - -	0,75697	0,395212	149.20. 6 *sf*	395212
Arcturus -	0,59847	1,488713	179.59.55,7 *sp*	1786455
Capella -	0,88905	0,506123	22.29.12,5 *nf*	632654
Lyra - -	0,36349	0,498949	40.29.14 *nf*	648634
Rigel - -	0,55470	0,709381	4.36.52 *np*	957665
α Orionis -	0,71410	0,842559	1.38.38 *np*	1137455
Procyon -	0,74161	0,523428	156.32.21 *sp*	732799
Aldebaran -	0,72736	0,608148	2.45.15 *nf*	851407
Pollux - -	0,78643	0,743971	50.12.11 *np*	1056439
Spica - -	0,74009	0,902004	7. 6.44 *np*	1298886
Antares -	0,74110	1,000835	0.16.10,5 *np*	1451219
Altair - -	0,64544	1,071042	40.48. 4 *nf*	1574431
Regulus -	0,75095	0,706833	17.43.53 *np*	1046113
β Leonis -	0,68003	0,443842	54.10.14,5 *np*	665763
β Tauri -	0,73063	0,633317	2. 5.15,5 *nf*	949976
Fomalhaut	0,66693	0,383414	13.22. 5,5 *nf*	575121
α Cygni -	0,46516	0,529503	0.18. 2,2 *np*	847204
Castor - -	0,55841	0,474647	11.30.32 *np*	949293
α Ophiuchi	0,35202	0,290934	8.23.43 *nf*	581869
α Coronæ -	0,23427	0,370580	37.21.17 *nf*	741160
α Aquarii -	0,55743	0,756754	4.38.19,5 *nf*	1513508
α Andromedæ	0,55889	0,464035	2.33.34 *nf*	928071
α Serpentis	0,38655	0,598458	6.38.54 *nf*	1196917
α Pegasi -	0,55567	0,734265	5.35.47,5 *nf*	1468530
α Hydræ -	0,46554	0,538281	17. 8.26 *np*	1238046
α² Libræ -	0,43377	0,563892	15. 4.29 *np*	1353342
γ Pegasi -	0,44540	0,618272	1.39.27 *nf*	1545679
α Arietis -	0,43893	0,342934	9.35.29,5 *nf*	857336
α Ceti -	0,33271	0,454165	11.26. 5,5 *np*	1271662
α Herculis	0,21909	0,446795	5.56.38,5 *nf*	1340388
β Virginis -	0,36039	0,967572	48.29. 2,5 *nf*	2902716
γ Aquilæ -	0,30898	0,502168	0.36.25 *nf*	1506503
α² Capricorni	0,31390	0,537285	19.51.52,5 *nf*	1880497
β Aquilæ -	0,24370	0,226458	96.36.59,5 *sp*	905830
α′ Capricorni	0,26151	0,519230	17. 4.54,5 *nf*	2180769
α′ Libræ -	0,17347	0,349371	26.29.44,5 *np*	2096229

By the contents of this Table, Fig. 5 is drawn with the lines contained in the third column and the angles of the fourth; the scale of it is that of the 5th and 3d figures; and it represents the directions and angular quantities of the real motions that are required to compound with the parallactic effects of the second column, so as to produce those annual proper motions which are established by observation.

Fig. 6 is drawn on the reduced scale of the 2d and 4th figures. The lines make the same angles with the direction of the solar motion as before, but their lengths are in the proportion of the velocities contained in the last column.

Remarks that lead to a necessary Examination of the Cause of the sidereal Motions.

The first particular that will strike us when we cast our eye on Fig. 5, is the uncommon arrangement of the stars. It seems to be a most unaccountable circumstance that their real motions should be as represented in that figure; indeed, if we except only ten of the stars, all the rest appear to be actuated by the same influence, and, like faithful companions of the sun, to join in directing their motions towards a similarly situated part of the heavens.

This singularity is too marked not to deserve an examination; for unless a cause for such peculiar directions can be shown to exist, I do not see how we can reconcile them with a certain equal distribution of situations, quantities, and motions, which our present investigation seems to demand. In order to penetrate as far as we can into this intricate subject, we shall take a general view of the causes of the motions of celestial bodies.

A motion of the stars may arise either from their mutual gravitation towards each other, or from an original projectile force impressed upon them. These two causes are known to act on all the bodies belonging to the solar system, and we may therefore reasonably admit them to exert their influence likewise on the stars. But it will not be sufficient to know a general cause for their motions, unless we can show that its influence will tend to make them go towards a certain part of the heavens rather than to any other. Let us examine how these causes are acting in the solar system.

The projectile motions of the planets, the asteroids, and the satellites, excepting those of the Georgium Sidus, are all decidedly in favour of a marked singularity of direction. We may add to them the comet of the year 1682, whose regular periodical return in 1759 has sufficiently proved its permanent connection with the solar system. Here then we have not less than 23 various bodies belonging to the solar system to show that this cause not only can, but in the only case of which we have a complete knowledge, actually does influence the celestial motions, so as to give them a very particular appropriate direction. Even the exception of the Georgian satellites may be brought in confirmation of the same peculiarity; for though they do not unite with the rest of the bodies of our system, they still conform among each other to establish the same tendency of a similar direction in their motion round the primary planet. And thus it is proved that the similar direction of the motion of a group of stars may be ascribed to their similar projectile motions without incurring the censure of improbability.

Let us however pursue the objection a little farther, and as

we have shown that the celestial bodies of the solar system actually have these similar projectile motions, it may be required that we should also prove that the stars have them likewise; since the appearances in Fig. 5 may otherwise be looked upon as merely the consequence of the assumed solar motion. To this I answer, that setting aside the solar motion, and allowing the observations of astronomers on the proper motion of the stars to give us the real direction and angular quantity of these motions, even then the same similarity will equally remain to be accounted for. In my examination of Fig. 1 and 3, it has been shown that we ought to ascribe the similar directions of the sidereal motions to some physical cause, which probably exerts its influence also on the solar motion; therefore in reverting to those figures I may be said to appeal to the actual state of the heavens, for a proof of what has been advanced, with respect to the similarity of the directions of projectile motions.

Having thus examined one cause of the sidereal motions, and shown that as far as we are acquainted with its mode of acting in the solar system, it is favourable to a similarity of direction; and that moreover, if we ascribe the motion of the stars to it, we have also good reason, from observation, to believe it to be in favour of the same similarity; we may in the next place proceed to consider the mutual gravitation of the stars towards each other. This is an acknowledged principle of motion, and the laws of its exertion being perfectly known, we shall in this inquiry meet with no difficulty relating to its direction, which is always towards the attracting body.

Considerations of the attractive Power required for a sufficient Velocity of the sidereal Motions.

As attraction is a power that acts at all distances, we ought to begin by examining whether the motions of our stars can be accounted for by the mutual gravitation of neighbouring stars towards each other, or by a periodical binal revolution of them about a common centre of gravity; or whether we ought not rather to have recourse to some very distant attractive centre. This may be decided by a calculation of the effects arising from the laws according to which the principle of attraction is known to act. For instance, let the sun and Sirius be two equal bodies placed in the most favourable situation to permit a mutual approach by attraction: that is, let them be without projectile motions, and removed from all other stars which might impede their progress towards each other, by opposite attractions. Then, by calculation, the space over which one of them would move in a year, were the matter of both collected in the other as an attractive centre, would be less than a five thousand millionth part of a second; supposing that motion to be seen by an eye at the distance of Sirius, and admitting the parallax of the whole orbit of the earth on this star to be one second.

This proves evidently that the mere attraction of neighbouring stars acting upon each other cannot be the cause of the sidereal motions that have been observed.

In the case of supposed periodical binal revolutions of stars about a common centre of gravity, where consequently projectile motions must be admitted, the united power of the connected stars, provided the mass of either of them did not

greatly exceed that of the sun, would fall very short of the attraction required to give a sufficient velocity to their motions. The star Arcturus for example, which happens to move, as is required, in an opposite direction to the proposed solar motion, were it connected with the sun, and had the proper degree of necessary projectile motion, could not describe an arch of 1″ of its orbit, about their common centre, in less than 102 years; and though the opposite motion of the sun, by a parallactic effect would double that quantity, it still would fall short of the change we observe in this star in the course of a single year.

Other considerations are still more against the admission of such partial connections: they would intirely oppose the similarity of the directions of the sidereal motions that have been proved to exist, and which we are now endeavouring to explain.

Let us then examine in what manner a distant centre of attraction may be the cause of the required motions. By admitting this centre to be at a great distance, we shall have its influence extended over a space that will take in a whole group of stars, and thus the similar directions of their motions will be accounted for. Their velocities also may be ascribed to the energy of the centre, which may be sufficiently great for all the purposes of the required motions. A circumstance, however, attends the directions of the motions to be explained, which shows that a distant centre of attraction alone will not be sufficient; for these motions, as we may see in Fig. 3, though pretty similar in their directions, still are diverging; whereas if they were solely caused by attraction, they would converge towards the attracting centre, and point

out its situation. It is therefore evident that projectile motions must be combined with attraction, and that the motions of the stars when regulated in this manner, are not unlike the disposition by which the bodies of the solar system are governed. If we pursue this arrangement, it will be proper to consider the condition, and probable existence of such a centre of attraction.

There are two ways in which a centre of attraction, so powerful as the present occasion would require, may be constructed; the most simple of them would be a single body of great magnitude; this may exist, though we should not be able to perceive it by any superiority of lustre; for notwithstanding it might have the usual starry brightness, the decrease of its light arising from its great distance would hardly be compensated by the size of its diameter; but to have recourse to an invisible centre, or at least to one that cannot be distinguished from a star, would be intirely hypothetical, and, as such, cannot be admitted in a discussion, the avowed object of which is to prove its existence.

The second way of the construction of a very powerful centre, may be joint attraction of a great number of stars united into one condensed group.

The actual existence of such groups of stars has already been proved by observations made with my large instruments; many of those objects, which were looked upon as nebulous patches, having been completely resolved into stars by my 40 and 20-feet telescopes. For instance, the nebula discovered by Dr. Halley in the year 1714, in which the discoverer, and other observers after him, have seen no star,

I have ascertained to be a globular cluster, containing, by a rough calculation, probably not less than fourteen thousand stars. From the known laws of gravitation, we are assured that this cluster must have a very powerful attractive centre of gravity, which may be able to keep many far distant celestial bodies in control.

But the composition of an attractive centre is not limited to one such cluster. An union of many of them will form a still more powerful centre of gravitation, whose influence may extend to a whole region of scattered stars. To prove that I argue intirely from observations, I shall mention that another nebula, discovered by Mr. MESSIER in the year 1781, is, by the same instruments, also proved to consist of stars; and though they are seemingly compressed into a much smaller space, and have also the appearance of smaller stars, we may fairly presume that these circumstances are only indications of a greater distance, and that, being a globular cluster, perfectly resembling the former, the distance being allowed for, it is probably not less rich in the number of its component stars. The distance of these two clusters from each other is less than 12 degrees, and we are certain that somewhere in the line joining these two groups there must be a centre of gravitation, far superior in energy to the single power of attraction that can be lodged in either of the clusters.

I have selected these two remarkable objects merely for their sinuation, which is very near the line of the direction of the solar motion; but were it necessary to bring farther proof of the existence of combined attractions, the numerous objects

of which I have given catalogues* would amply furnish me with arguments.

If a still more powerful but more diffused exertion of attraction should be required than what may be found in the union of clusters, we have hundreds of thousands of stars, not to say millions, contained in very compressed parts of the milky way, some of which have already been pointed out in a former Paper.† Many of these immense regions may well occasion the sidereal motions we are required to account for; and a similarity in the direction of these motions will want no illustration.

With regard to the situation of the condensed parts of the milky way, and of the two clusters that have been mentioned, we must remark, that the seat of attraction may be in any part of the heavens whatsoever; for when projectile motions are given to bodies that are retained by an attractive centre, they may have any direction, even that at right angles to its situation not excepted.

It will give additional force to the arguments I have used for the admission of far distant centres of attraction, as well as projectile motions in the stars that are connected with them, when we take notice that, independent of the solar motion, and setting that intirely aside, the action of these causes will be equally required to explain the acknowledged proper motions of the stars. For if the sun be at rest, then Arcturus must actually change its place more than 2″ a year, and consequently this and many other stars, which are well known to change their situation, must be supposed to have

* Phil. Trans. for 1786, page 457; for 1789, page 212; for 1802, page 477.

† Ibid. for 1802, page 495.

projectile motions, and to be subject to the attraction of far distant centres.

Determination of the Quantity of the Solar Motion.

If I am not mistaken, it will now be allowed that no objection can arise against any solar velocity we may fix upon, for want of a cause that may be assigned to act upon the sun, and many stars, so as to account for their motions, and similar tendency towards a certain part of the heavens; we may consequently proceed in examining whether the quantity that has been assumed for calculating the contents of the XIth Table, will sufficiently come up to the conditions we have adopted for directing our determination.

In Fig. 6 we have the velocities of the 36 stars delineated, and by examining the last column of the Table from which they are taken, we find that the parallactic effects arising from the proposed solar motion require the velocity of 18 stars to exceed that of the sun, and exactly the same number to be inferior to it; so far then the rank which has been assigned to the solar motion is a perfect medium among the sidereal velocities.

If we examine in the next place how this motion will agree with a mean rate deduced from the velocities in the above mentioned column, we find a 36th part of their sum to be 1196550. A solar motion, therefore, which agrees with this mean rate will differ from one assigned by the middle rank no more than 0",079558; and, on account of the smallness of this quantity, the calculations required to lessen it, by some little increase of the solar motion, might well be dispensed with; but if we were desirous of greater precision, the

secondary purpose, next to be considered, would rather incline us to an opposite alteration.

The great disparity of the sidereal motions, which has been mentioned as an incongruity in the first part of this Paper, and has more evidently been shown to exist when we examined the representations of these motions in the 3d figure, is the next point we have to consider in the effect of the solar motion. Let us see how far we have been successful in lessening the ratio these velocities bear to each other. The last column of the Xth Table contains them as they must have been admitted if the sun had been at rest. The proportion of the quickest motion to the slowest is there as 2504621 to 103036; and the velocity of one is therefore above 24 times greater than that of the other. But in consequence of the solar motion we have used, the two extreme velocities are reduced to 2902716 and 395212; which gives a proportion of less than $7\frac{1}{2}$ to 1.

If the quantity of the solar motion were lessened to 1″, we might bring the ratio of the extreme velocities so low as 6 to 1; but as the middle rank has already given it a little below the mean rate, I do not think that we ought to lower it still more; so that when all circumstances are properly considered, there is a great probability that the quantity assumed in the last calculation may not be far from the truth. It appears, therefore, that in the present state of our knowledge of the observed proper motions of the stars, we have sufficient reason to fix upon the quantity of the solar motion to be such as by an eye placed at right angles to its direction, and at the distance of Sirius from us, would be seen to describe annually an arch of 1″,116992 of a degree; and its

velocity, till we are acquainted with the real distance of this star, can therefore only be expressed by the proportional number of 1116992.

Concluding Remarks and Inferences.

We have now only to notice a few remarks that may be made, by way of objection to the solar motion I have fixed upon. If the quantity of this motion is to be assigned by the mean rank of sidereal velocities, it may be asked, will not the addition of every star, whose proper motion shall be ascertained, destroy that middle rank, which has been established? To this I shall answer, that future observations may certainly afford us more extensive information on the subject, and even show that the solar motion should not exactly hold that middle rank, which from various motives we have been induced to assign to it; but at present it appears, that according to the doctrine of chances, a middle rank among the sidereal velocities must be the fairest choice, and will remain so, unless, what is now a secondary consideration, should hereafter become of more importance than the first. That this should happen is not impossible, when a general knowledge of the proper motions of all the stars of the 1st, 2d, and 3d magnitudes can be obtained; but then the method of calculation that has been traced out in this and the former Paper, is so perfectly applicable to any new lights observation may throw upon the subject, that a more precise and unobjectionable solar motion can be ascertained by it with great facility. Hitherto we find that a mean rank agrees sufficiently with the phenomena that were to be explained: the apparent velocities of Arcturus and Aldebaran, without a solar motion

for instance, were to each other, in the IXth Table, as 208 to 12; our present solar motion has shown, that when the deception arising from its parallactic effect is removed by calculation, these velocities are to each other only as 179 to 85, or as 2 to 1. And though Arcturus still remains a star that moves with great velocity, yet in the XIth Table we have 4 or 5 stars with nearly as much motion; and 4 with more.

Our solar motion also removes the deception by which the motion of a star of the consequence of α Orionis is so concealed as hardly to show any velocity; whereas by computation we find that it really moves at a rate which is fully equal to the motion of the sun.

I must now observe, that the result of calculations founded upon facts, such as we must admit the proper motions of the stars to be, should give us some useful information, either to satisfy the inquisitive mind, or to lead us on to new discoveries. The establishment of the solar motion answers both these ends. We have already seen that it resolves many difficulties relating to the proper motions of the stars, and reconciles apparent contradictions; but our inquiries should not terminate here. We are now in the possession of many concealed motions, and to bring them still more to light, and to add new ones by future observations should become the constant aim of every astronomer.

This leads me to a subject, which though not new in itself, will henceforth assume a new and promising aspect. An elegant outline of it has long ago been laid before the public in a most valuable paper on general Gravitation, under

the form of "Thoughts" on the subject;* but I believe, from what has been said in this Paper, it will now be found that we are within the reach of a link of the chain which connects the principles of the solar and sidereal motions with those that are the cause of orbitual ones.

A discovery of so many hitherto concealed motions, presents us with an interesting view of the construction of that part of the heavens which is immediately around us. The similarity of the directions of the sidereal motions is a strong indication that the stars, having such motions, as well as the sun, are acted upon by some connecting cause, which can only be attraction; and as it has been proved that attraction will not explain the observed phenomena without the existence of projectile motions, it must be allowed to be a necessary inference, that the motions of the stars we have examined are governed by the same two ruling principles which regulate the orbitual motions of the bodies of the solar system. It will also be admitted that we may justly invert the inference, and from the operation of these causes in our system, conclude that their influence upon the sidereal motions will tend to produce a similar effect; by which means the probable motion of the sun, and of the stars in orbits, becomes a subject that may receive the assistance of arguments supported by observation.

What has been said in a paragraph of a former Paper, where the sun is placed among the insulated stars,† does not contradict the present idea of its making one of a very extensive system. On the contrary, a connection of this na-

* See the note Phil. Trans. for 1783, page 283. † Ibid. for 1802, page 478.

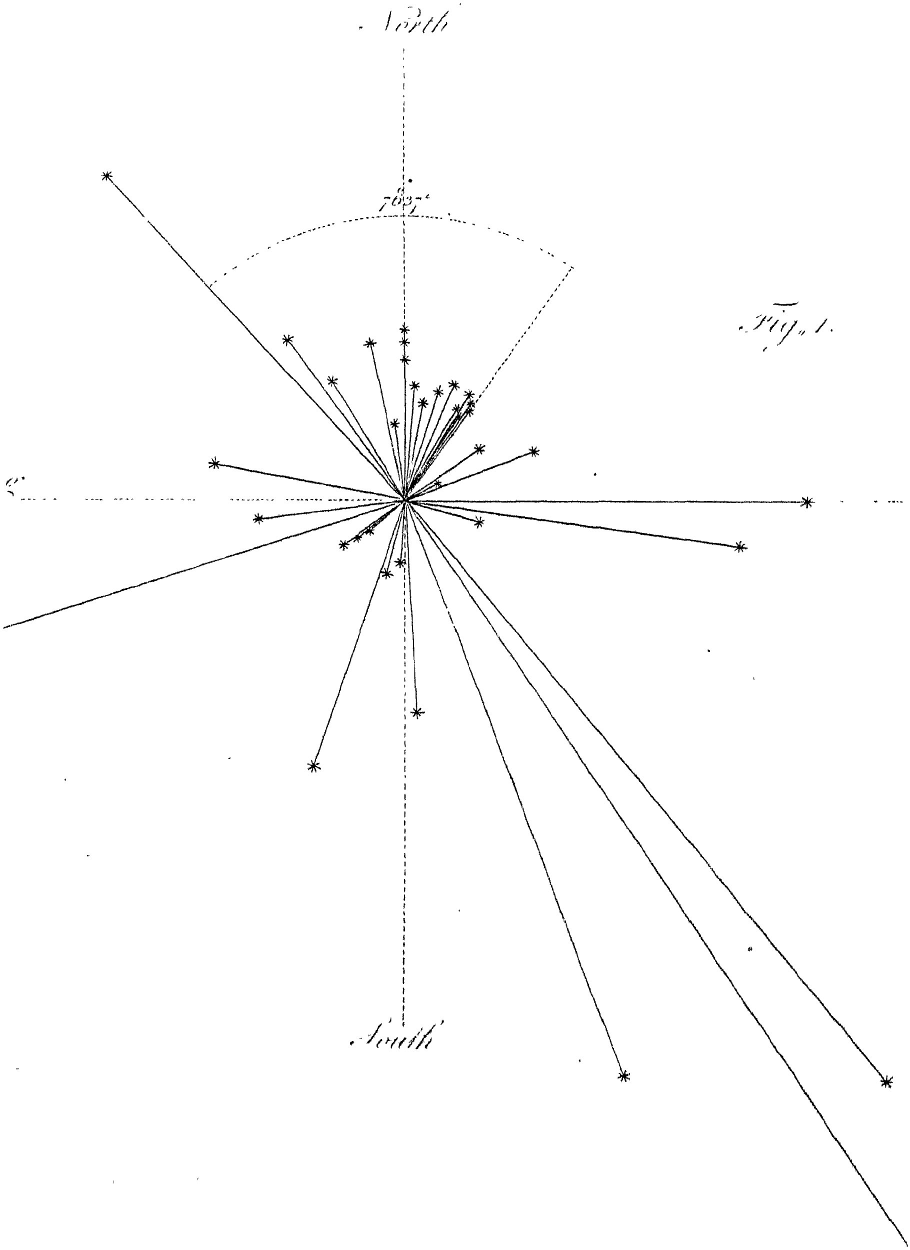
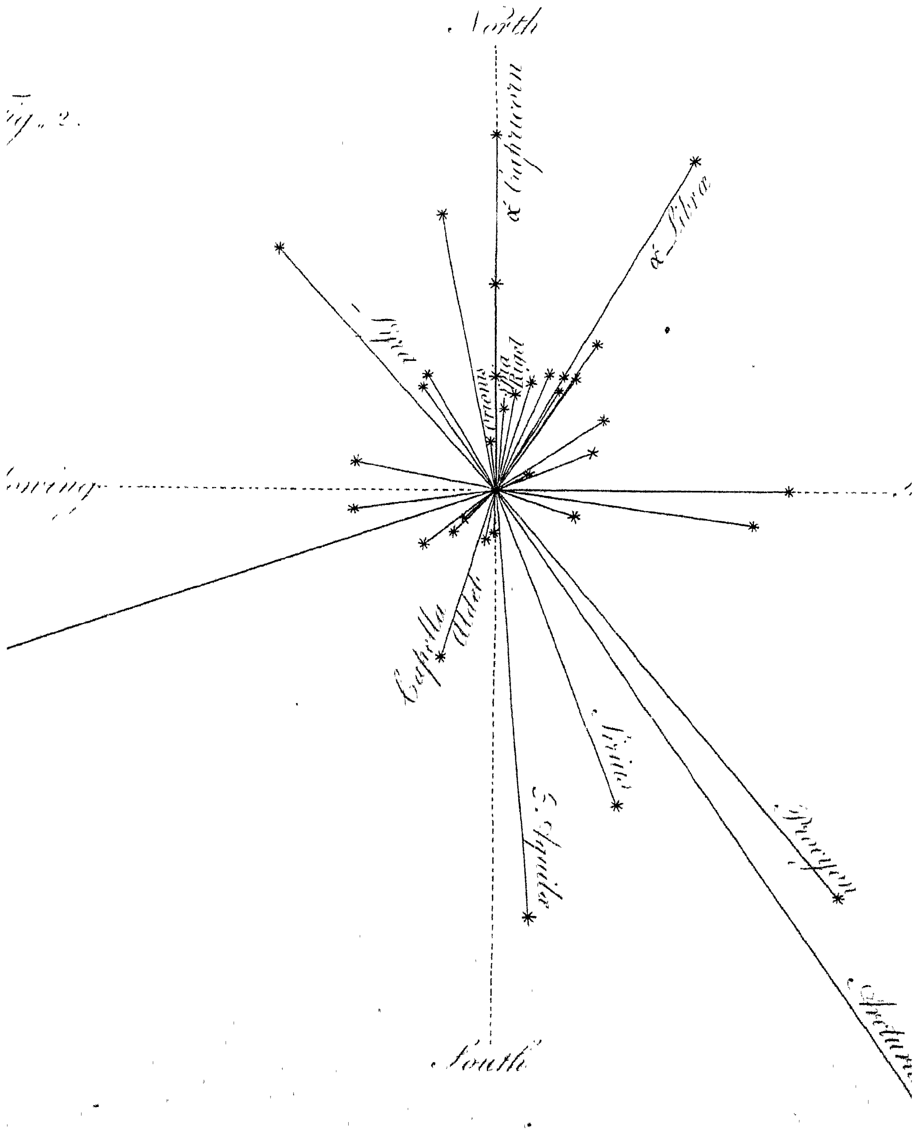


Fig. 1.


North
α Capricorn
α Libra
Orionis
Spica
Rigel
Capella
Aldeb
Virgo
ζ Aquila
Procyon
Arcturus
South

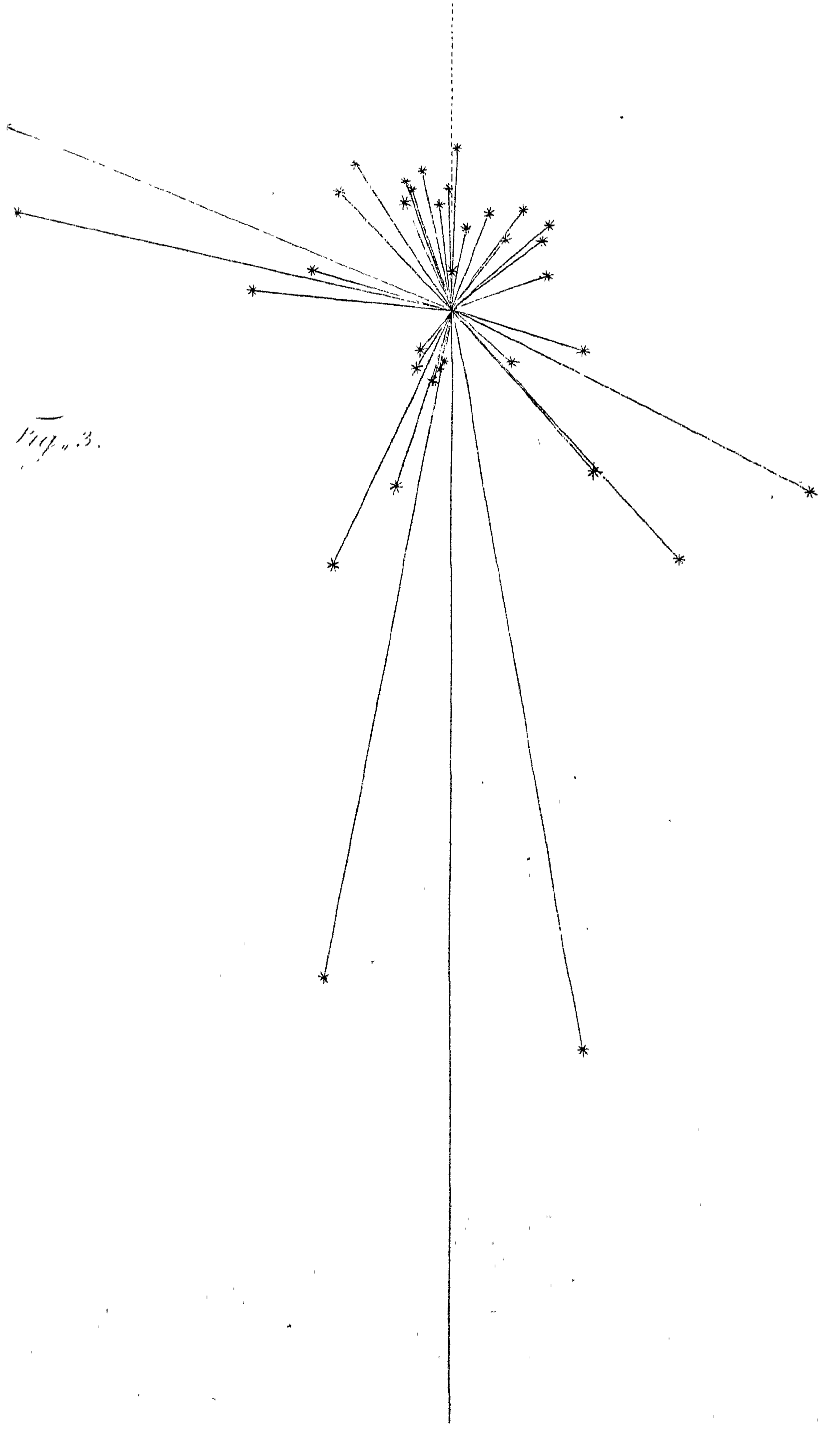
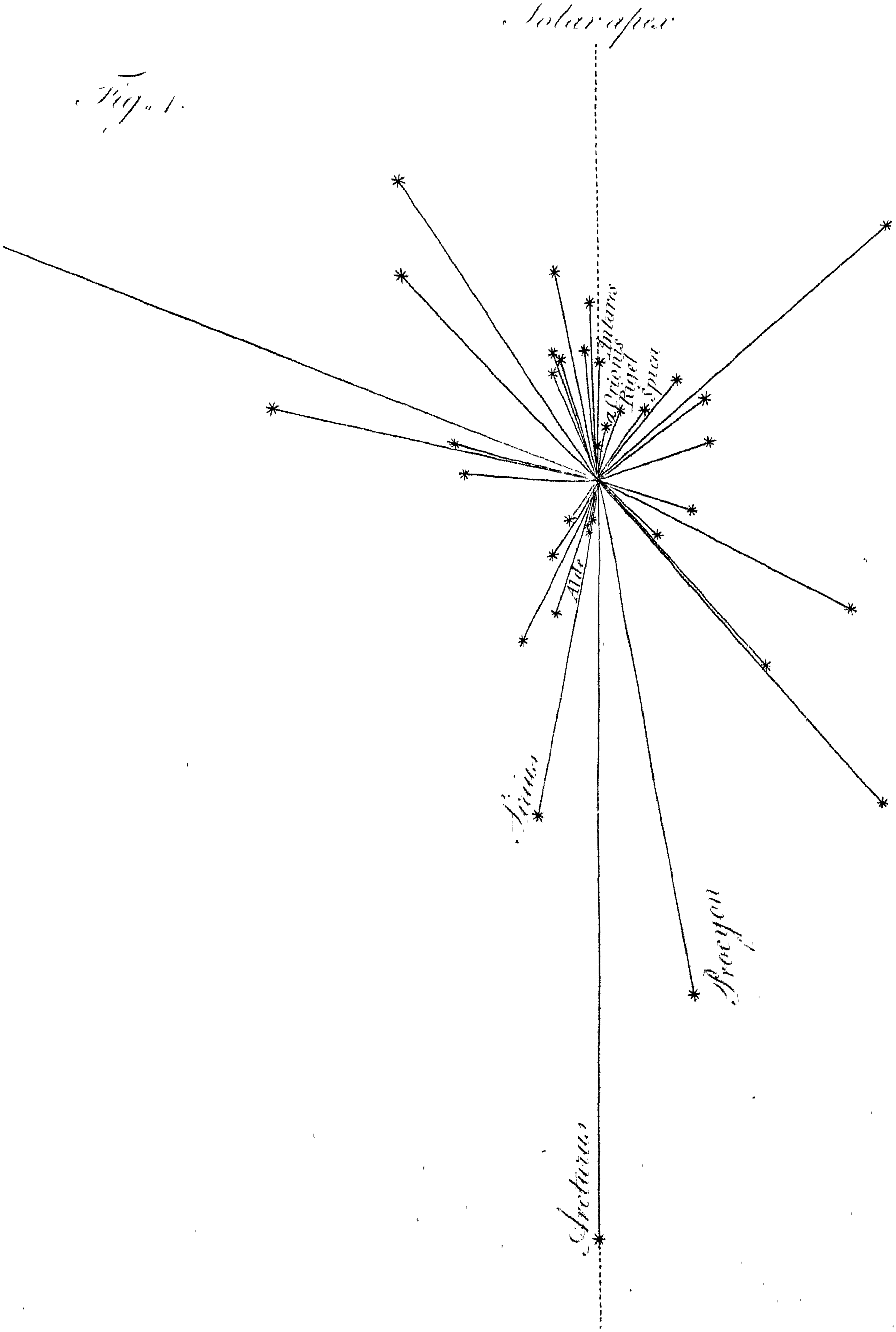


Fig. 3.

Fig. 1.


Solar apex
Antares
α Orionis
Rigel
Spica
Alde
Sirius
Procyon
Arcturus

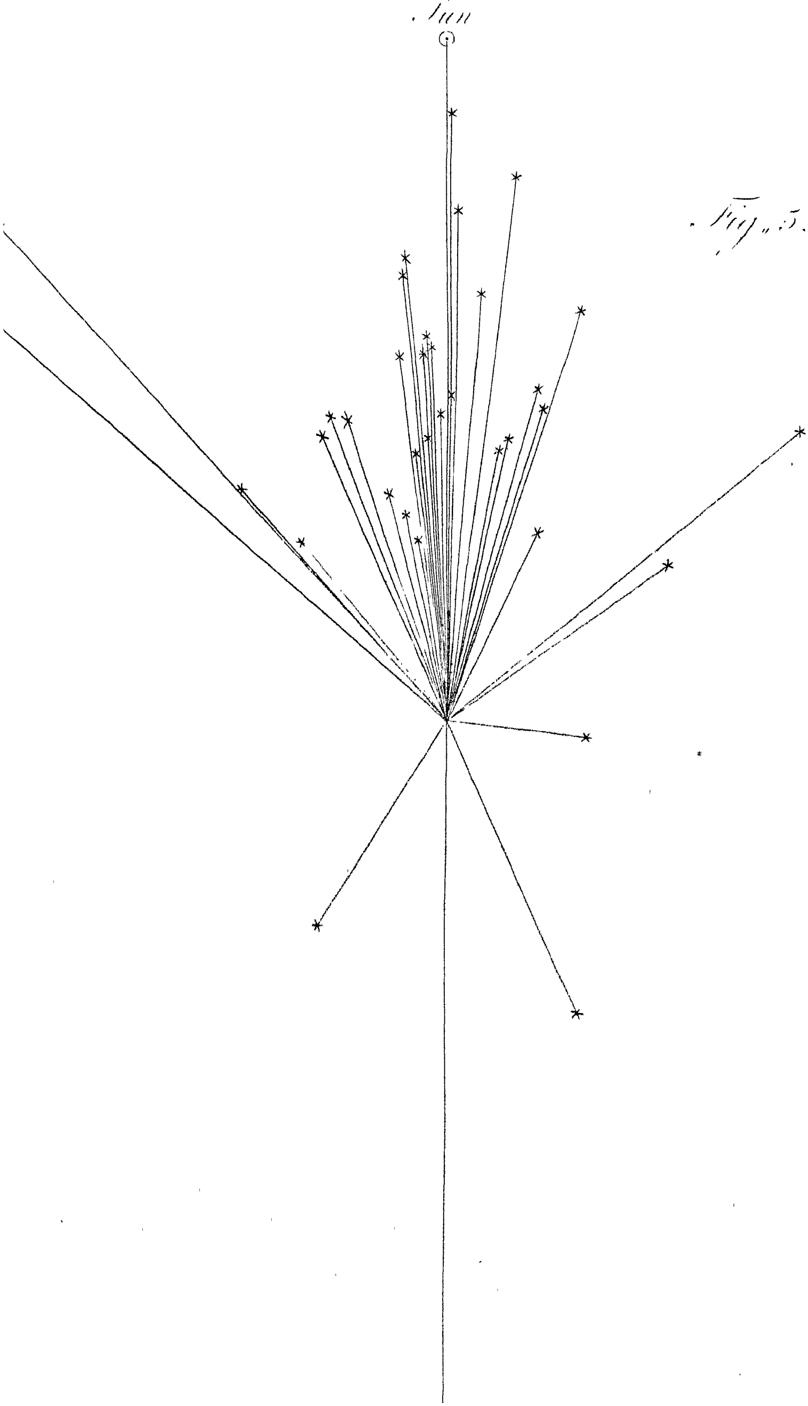
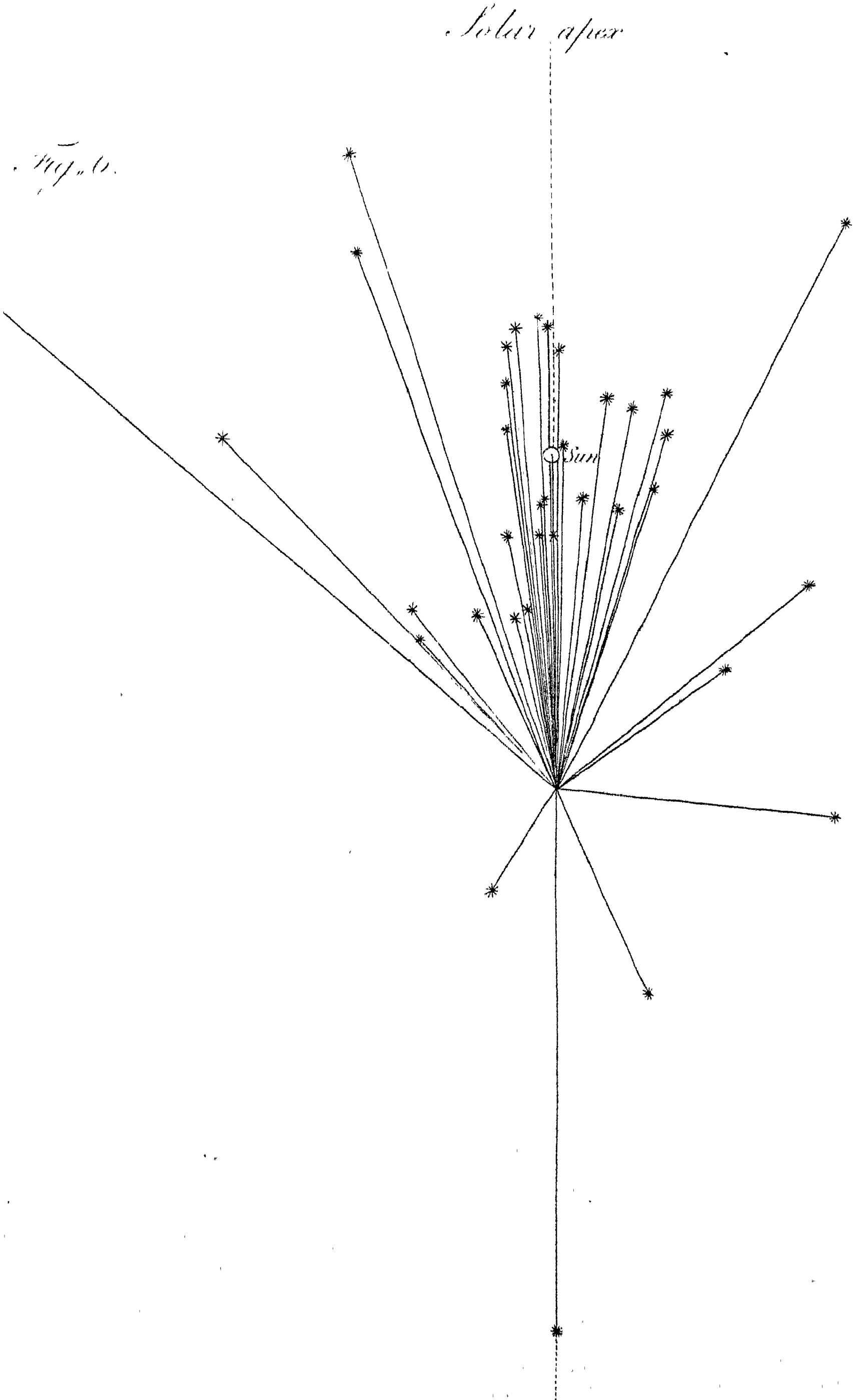


Fig. 5.

Fig. 6.



ture has been alluded to in the same Paper.* The insulation ascribed to the sun relates merely to a supposed binary combination with some neighbouring star; and it has now been proved by an example of Arcturus, that the solar motion cannot be occasioned or accounted for by a periodical revolution of the sun and this or any other star about their common centre of gravity.

* Phil. Trans. for 1802, page 479.

ERRATA

In Table VII. of the first part of this Paper, star Aldebaran, the two last columns,
for 13° 18′ 58″, *read* 13° 41′ 48′.
for 0,02842, *read* 0,02922.

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METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1805.

1805	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Jan. 1	29	8	0	29	44	29,85	79		NE	2	Cloudy.
	33	2	0	33	47	29,80	80		ENE	2	Cloudy.
2	33	8	0	37	45	29,75	94	0,030	E	1	Foggy.
	40	2	0	40	47	29,78	95		E	1	Foggy.
3	37	8	0	37	46	30,01	95	0,045	E	1	Foggy.
	41	2	0	41	49	30,05	95		E	1	Cloudy.
4	38	8	0	40	47	30,07	95	0,040	ENE	1	Cloudy.
	43	2	0	43	52	30,07	91		SE	1	Fair.
5	37	8	0	40	48	29,96	93		ESE	1	Cloudy.
	43	2	0	43	49	29,91	93		SE	1	Cloudy.
6	41	8	0	43	49	29,93	94	0,016	SSE	1	Cloudy.
	45	2	0	45	52	30,00	93		SW	1	Cloudy.
7	41	8	0	46	50	29,78	94		S	2	Cloudy.
	48	2	0	48	52	29,84	93		WNW	2	Cloudy.
8	33	8	0	33	49	30,40	90	0,110	WNW	1	Fine.
	42	2	0	42	53	30,41	90		SW	1	Fine.
9	34	8	0	35	50	30,35	92		SW	1	Foggy.
	35	2	0	34	52	30,31	92		SW	1	Foggy.
10	25	8	0	26	48	30,16	90		ENE	1	Cloudy.
	30	2	0	30	51	30,07	92		NE	1	Cloudy.
11	29	8	0	34	49	29,95	92		NE	1	Foggy.
	34	2	0	32	51	29,85	90		E	1	Cloudy.
12	27	8	0	27	47	29,58	87		ENE	1	Cloudy.
	32	2	0	32	49	29,40	88		E	1	Cloudy.
13	32	8	0	39	48	29,09	90		SE	2	Cloudy.
	44	2	0	44	51	28,98	91		SE	2	Cloudy.
14	39	8	0	39	48	28,88	93	0,350	SSW	1	Cloudy.
	44	2	0	44	51	29,04	90		S	1	Cloudy.
15	36	8	0	36	49	29,24	93		SE	1	Fair.
	41	2	0	41	52	29,37	91		S	1	Cloudy.
16	35	8	0	38	48	29,72	93	0,050	S	1	Fair.
	47	2	0	47	51	29,68	90		S	2	Cloudy.

METEOROLOGICAL JOURNAL
for January, 1805.

1805	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. A. M.	Therm. within. P. M.	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°			°	°		°				
Jan. 17	44	8	0	44	50	29,55	95	0,140	SSW	1	Fair.
	48	2	0	48	54	29,55	87		SSW	1	Fair.
18	34	8	0	34	50	29,70	91	0,165	SSW	1	Fine.
	41	2	0	40	53	29,76	87		WSW	1	Cloudy.
19	32	8	0	33	50	29,84	90		S	1	Fine.
	42	2	0	42	52	29,65	89		SSE	2	Cloudy.
20	37	8	0	41	50	28,98	93	0,110	ESE	1	Cloudy.
	45	2	0	45	52	28,89	94		SE	1	Rain.
21	33	8	0	33	50	29,14	94	0,112	WNW	1	Fine.
	40	2	0	40	54	29,20	91		NE	1	Cloudy.
22	32	8	0	34	50	29,10	93	0,033	NE	1	Snow.
	36	2	0	36	52	29,07	91		NE	1	Cloudy.
23	33	8	0	34	50	29,15	93	0,052	NE	1	Cloudy.
	36	2	0	36	52	29,31	93		NE	1	Snow.
24	32	8	0	32	49	29,61	91		ENE	2	Cloudy.
	33	2	0	33	50	29,64	89		NE	2	Cloudy.
25	31	8	0	34	48	29,68	88		NE	2	Cloudy.
	37	2	0	37	49	29,72	86		NE	2	Cloudy.
26	29	8	0	30	47	29,77	84		NE	2	Cloudy.
	31	2	0	30	48	29,77	83		NE	2	Cloudy.
27	29	8	0	30	46	29,75	84		NE	2	Cloudy.
	32	2	0	31	49	29,72	83		NE	2	Cloudy.
28	29	8	0	30	47	29,61	90		ENE	1	Cloudy.
	30	2	0	29	48	29,52	90		E	1	Cloudy.
29	29	8	0	32	46	29,18	92		ENE	1	Snow.
	37	2	0	36	49	29,04	87		ENE	1	Cloudy.
30	34	8	0	35	47	28,90	94	0,200	ENE	1	Cloudy.
	38	2	0	38	49	28,95	94		E	1	Cloudy.
31	34	8	0	35	47	29,21	93	0,060	E	1	Cloudy.
	37	2	0	37	50	29,38	91		NE	1	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1805.

1805	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Feb. 1	31	7	0	31	46	29,64	88		NE	2	Cloudy.
	33	2	0	32	48	29,70	83		NE	2	Cloudy.
2	23	7	0	23	45	29,85	85		NE	2	Fine.
	36	2	0	34	49	29,88	84		NW	1	Fair.
3	27	7	0	28	45	30,07	84		W	1	Cloudy.
	39	2	0	39	48	30,04	85		W	1	Fair.
4	34	7	0	37	46	29,84	93		W	1	Rain.
	48	2	0	44	49	29,51	94		S	1	Rain.
5	44	7	0	47	48	28,94	97	0,502	S	1	Cloudy.
	48	2	0	45	50	28,86	87		W	2	Cloudy.
6	31	7	0	31	47	29,66	83	0,030	NNE	2	Fine.
	37	2	0	37	49	29,85	76		NW	2	Fine.
7	31	7	0	31	47	29,95	87		W	1	Fair.
	44	2	0	44	50	29,83	83		SSW	2	Cloudy.
8	38	7	0	43	48	29,57	95	0,170	SSW	1	Cloudy.
	51	2	0	50	52	29,68	92		SW	1	Cloudy.
9	48	7	0	50	50	29,76	95	0,092	SSW	2	Cloudy.
	54	2	0	54	53	29,84	90		SW	2	Cloudy.
10	48	7	0	48	52	29,81	94		SW	1	Cloudy.
	53	2	0	53	55	29,72	88		SSW	1	Cloudy.
11	43	7	0	43	53	29,69	92		SSW	1	Cloudy.
	45	2	0	43	54	29,76	91		NE	1	Rain.
12	35	7	0	36	51	29,91	80		NE	2	Cloudy.
	41	2	0	41	55	30,01	81		NNE	2	Fair.
13	34	7	0	35	51	30,21	87		N	1	Cloudy.
	40	2	0	40	54	30,22	80		NW	1	Cloudy.
14	30	7	0	30	51	30,07	85		W	1	Fine.
	41	2	0	41	54	30,03	83		NW	1	Fine.
15	36	7	0	36	52	30,03	84		W	1	Cloudy.
	40	2	0	40	53	30,07	83		N	1	Cloudy.
16	32	7	0	33	50	30,13	86		E	1	Cloudy.
	41	2	0	41	53	30,07	80		E	1	Cloudy.

METEOROLOGICAL JOURNAL
for February, 1805.

1805	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Feb. 17	28	7	0	28	50	29,93	83		ENE	1	Fine.
	39	2	0	39	53	29,88	81		E	1	Fine.
18	32	7	0	33	50	29,82	85		E	1	Cloudy.
	40	2	0	40	54	29,83	78		ENE	1	Fine.
19	29	7	0	29	49	29,90	87		NE	1	Fine.
	41	2	0	41	53	29,96	82		NE	1	Fair.
20	29	7	0	29	49	30,04	88		NW	1	Fine.
	43	2	0	43	53	30,00	86		SW	1	Hazy.
21	39	7	0	39	50	29,90	95	0,085	SSW	1	Cloudy.
	51	2	0	48	54	29,74	95		S	2	Rain.
22	43	7	0	43	53	29,63	91	0,020	SW	1	Cloudy.
	51	2	0	51	55	29,72	76		SW	2	Fair.
23	40	7	0	40	53	29,91	86		WSW	2	Fine.
	50	2	0	50	55	30,07	75		W	2	Fair.
24	45	7	0	47	54	30,00	94	0,048	SSW	2	Cloudy.
	50	2	0	50	56	29,75	92		SSW	2	Rain.
25	39	7	0	39	54	29,78	90	0,098	W	2	Fine.
	50	2	0	50	57	29,83	76		W	2	Fair.
26	45	7	0	46	55	29,78	86		W	2	Cloudy.
	52	2	0	52	58	29,85	77		W	2	Cloudy.
27	43	7	0	43	56	29,70	90		SW	1	Cloudy.
	51	2	0	50	58	29,64	86		SSW	1	Cloudy.
28	46	7	0	46	56	29,50	80		W	2	Cloudy.
	49	2	0	47	58	29,58	76		WNW	2	Cloudy.

METEOROLOGICAL JOURNAL

for March, 1805.

1805	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
	°			°		°	°				
Mar. 1	38	7	0	40	55	29,40	81		WNW	2	Cloudy.
	46	2	0	44	57	29,52	80		NW	2	Fair.
2	34	7	0	35	54	29,88	88		WNW	1	Fair.
	47	2	0	46	56	29,99	80		WNW	2	Cloudy.
3	35	7	0	37	54	30,15	88	0,055	W	1	Cloudy.
	47	2	0	47	56	30,18	77		SW	1	Fair.
4	44	7	0	47	54	30,04	93	0,070	SW	1	Rain.
	54	2	0	53	57	30,04	80		W	2	Cloudy.
5	45	7	0	47	55	29,88	87		WSW	2	Cloudy.
	53	2	0	53	57	29,94	73		NW	2	Fair.
6	39	7	0	40	53	30,04	85		W	1	Fair.
	52	2	0	52	56	30,01	73		NW	2	Fair.
7	38	7	0	38	53	30,21	80		NE	1	Cloudy.
	43	2	0	43	55	30,24	74		ESE	2	Cloudy.
8	36	7	0	36	52	30,08	77		E	2	Cloudy.
	44	2	0	43	56	30,00	75		SE	2	Fair.
9	31	7	0	31	52	29,77	81		E	1	Fine.
	40	2	0	39	54	29,65	76		E	1	Hazy.
10	33	7	0	34	51	29,54	75		NE	1	Cloudy.
	42	2	0	42	54	29,57	73		NE	1	Fair.
11	30	7	0	31	51	29,86	81		E	1	Fine.
	46	2	0	46	53	29,93	74		S	1	Fine.
12	40	7	0	49	52	29,97	90		S	2	Rain.
	61	2	0	61	55	29,75	73		S	2	Fair.
13	48	7	0	48	54	30,05	87		SSW	2	Fine.
	62	2	0	62	57	30,02	70		S	2	Fine.
14	50	7	0	50	55	29,82	81		S	2	Fine.
	60	2	0	60	58	29,70	75		S	2	Fine.
15	44	7	0	44	57	29,77	85		SSW	1	Fine.
	55	2	0	55	58	29,85	75		NW	2	Cloudy.
16	39	7	0	40	57	29,97	84		WSW	1	Fine.
	54	2	0	53	59	29,97	78		SSW	1	Fair.

METEOROLOGICAL JOURNAL
for March, 1805.

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		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
Mar. 17	43°	7	0	43°	56°	29,87	85°		S	2	Cloudy.
	56	2	0	56	60	29,77	73		S	2	Fine.
18	46	7	0	46	58	29,90	87	0,057	SW	1	Rain.
	51	2	0	51	58	29,97	88		SE	1	Cloudy.
19	40	7	0	40	57	30,19	84		NNE	2	Cloudy.
	52	2	0	52	60	30,20	69		NE	2	Fine.
20	41	7	0	41	57	30,13	90	0,205	NE	1	Rain.
	45	2	0	45	58	30,13	90		ENE	1	Rain.
21	42	7	0	42	56	30,10	90	0,225	NE	1	Cloudy.
	48	2	0	47	59	30,06	78		NE	1	Fair.
22	39	7	0	40	56	29,96	87		NE	1	Cloudy.
	50	2	0	48	58	29,96	82		E	1	Fair.
23	39	7	0	39	56	30,00	84		NE	2	Fine.
	49	2	0	48	58	30,07	77		ENE	2	Fine.
24	35	7	0	37	56	30,21	83		ENE	2	Fine.
	47	2	0	47	57	30,22	73		ENE	2	Fine.
25	31	7	0	32	55	30,14	84		NE	2	Fine.
	47	2	0	47	58	30,02	77		NE	1	Fine.
26	32	7	0	33	55	29,90	85		NE	1	Fine.
	48	2	0	48	57	29,90	76		E	1	Fine.
27	34	7	0	36	54	29,92	83		ENE	1	Cloudy.
	43	2	0	42	56	29,97	80		NE	1	Cloudy.
28	29	7	0	30	52	30,12	78		NE	1	Fine.
	41	2	0	40	55	30,15	73		NE	1	Fair.
29	31	7	0	34	51	30,13	77		NE	1	Cloudy.
	43	2	0	41	53	30,08	82		S	1	Rain.
30	39	7	0	46	52	29,93	92	0,270	SSW	1	Cloudy.
	57	2	0	57	54	29,91	91		SW	1	Cloudy.
31	45	7	0	46	54	29,87	91		SW	1	Fine.
	57	2	0	56	56	29,81	73		S	1	Cloudy.

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1805	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. A.M.	Therm. within. P.M.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
	°			°	°		°				
April 1	47	7	0	48	54	29,72	88	0,080	SE	1	Rain.
	57	2	0	57	57	29,78	74		WNW	1	Fair.
2	47	7	0	47	55	29,97	83		NNE	1	Fair.
	59	2	0	58	59	29,98	71		N	1	Fair.
3	46	7	0	48	57	29,98	83		W	1	Cloudy.
	57	2	0	57	59	29,96	68		WNW	1	Fair.
4	45	7	0	45	57	29,47	83	0,170	SW	2	Cloudy.
	49	2	0	47	57	29,47	83		W	2	Rain.
5	38	7	0	40	55	29,64	85	0,045	W	2	Fine.
	51	2	0	48	58	29,59	77		NNW	2	Cloudy.
6	40	7	0	41	56	29,67	86		WNW	1	Rain.
	48	2	0	47	57	29,80	79		NNE	1	Cloudy.
7	43	7	0	43	56	30,16	85	0,088	NE	2	Cloudy.
	53	2	0	52	59	30,24	76		NE	2	Fair.
8	37	7	0	39	56	30,38	87		NE	1	Foggy.
	53	2	0	52	58	30,38	75		ENE	1	Fine.
9	38	7	0	40	56	30,38	85		E	1	Fair.
	54	2	0	54	59	30,34	73		E	1	Fine.
10	39	7	0	41	56	30,27	83		E	1	Fair.
	55	2	0	55	58	30,18	76		E	1	Hazy.
11	41	7	0	43	56	30,00	83		E	1	Hazy.
	58	2	0	57	58	29,92	73		E	1	Hazy.
12	42	7	0	44	56	29,81	81		ESE	1	Fine.
	62	2	0	61	59	29,76	65		S	1	Fine.
13	42	7	0	46	57	29,67	74		SE	1	Hazy.
	64	2	0	63	61	29,61	64		S	1	Fine.
14	47	7	0	48	58	29,45	77		E	1	Hazy.
	59	2	0	59	60	29,41	70		E	2	Fair.
15	43	7	0	45	58	29,44	83		NE	1	Fine.
	55	2	0	54	59	29,42	76		NE	2	Cloudy.
16	43	7	0	45	57	29,50	82		NNW	2	Cloudy.
	53	2	0	49	58	29,52	76		NW	2	Cloudy.

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1805	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Apr. 17	40	7	0	43	56	29,72	85		NNE	2	Cloudy.
	50	2	0	48	57	29,82	81		NE	2	Cloudy.
18	45	7	0	46	57	29,95	85		NNW	1	Cloudy.
	56	2	0	54	58	29,98	68		NNW	1	Cloudy.
19	45	7	0	47	57	30,05	83		NE	1	Fair.
	58	2	0	57	59	30,08	69		NE	1	Fair.
20	38	7	0	43	57	30,20	84		ENE	1	Hazy.
	61	2	0	58	59	30,20	70		NE	1	Hazy.
21	45	7	0	47	58	30,20	81		WSW	1	Fine.
	63	2	0	62	59	30,15	68		NE	1	Fair.
22	43	7	0	44	58	30,11	82		NE	1	Rain.
	54	2	0	53	59	30,08	69		E	1	Fair.
23	36	7	0	40	57	30,11	79		NE	1	Fine.
	52	2	0	51	59	30,04	69		NE	2	Cloudy.
24	36	7	0	38	56	29,68	81		ENE	1	Hazy.
	52	2	0	52	57	29,49	68		E	2	Cloudy.
25	42	7	0	45	56	29,31	87	0,075	E	1	Cloudy.
	55	2	0	52	58	29,37	85		ENE	1	Rain.
26	42	7	0	43	56	29,44	91	0,330	NE	1	Cloudy.
	57	2	0	56	59	29,45	76		NE	1	Fair.
27	44	7	0	44	56	29,65	86		N	1	Cloudy.
	52	2	0	51	57	29,73	75		N	1	Cloudy.
28	37	7	0	38	56	29,76	86	0,090	NE	1	Rain.
	46	2	0	45	56	29,82	77		NE	1	Cloudy.
29	35	7	0	35	55	29,55	88	0,165	E	2	Snow.
	44	2	0	42	56	29,33	92		NE	2	Rain.
30	36	7	0	37	54	29,65	85	0,540	NE	1	Fair.
	50	2	0	50	57	29,70	69		NE	1	Fair.

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1805	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. A. M.	Therm. within. P. M.	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Str.	Weather.
	°			°	°		°				
May 1	37	7	0	39	55	29,67	86		W	1	Fair.
	53	2	0	53	57	29,61	69		W	1	Cloudy.
2	40	7	0	42	55	29,57	76		NE	1	Cloudy.
	53	2	0	50	57	29,60	75		NE	1	Hazy.
3	38	7	0	42	55	29,67	87		NE	1	Hazy.
	55	2	0	55	58	29,62	71		E	1	Fair.
4	40	7	0	42	56	29,77	86		E	1	Hazy.
	57	2	0	57	58	29,82	67		SW	2	Cloudy.
5	46	7	0	46	56	29,88	83	0,033	W	2	Cloudy.
	57	2	0	55	58	29,90	73		WNW	1	Cloudy.
6	40	7	0	44	56	30,04	83	0,025	W	1	Fine.
	62	2	0	61	58	30,02	69		SW	1	Cloudy.
7	48	7	0	50	57	29,95	84		SW	2	Cloudy.
	59	2	0	59	58	29,90	77		SSW	1	Rain.
8	49	7	0	51	57	29,57	89	0,275	S	1	Rain.
	53	2	0	49	58	29,50	87		N	2	Rain.
9	40	7	0	42	57	29,65	85	0,185	N	2	Fair.
	53	2	0	53	58	29,72	75		N	2	Fair.
10	45	7	0	47	57	29,63	88		SW	2	Rain.
	59	2	0	57	59	29,56	74		W	1	Cloudy.
11	48	7	0	50	57	29,39	83	0,023	SW	2	Cloudy.
	57	2	0	56	60	29,33	75		WSW	2	Rain.
12	42	7	0	45	57	29,45	83	0,165	WSW	2	Fair.
	57	2	0	54	59	29,61	74		WSW	2	Fair.
13	42	7	0	46	57	30,01	82		WNW	1	Fine.
	60	2	0	59	59	30,10	68		N	1	Fair.
14	43	7	0	46	58	30,19	83		NNE	1	Fine.
	58	2	0	58	60	30,13	71		NNE	1	Cloudy.
15	40	7	0	45	57	29,94	82		NE	1	Hazy.
	58	2	0	55	59	29,83	75		NE	1	Cloudy.
16	48	7	0	49	58	29,75	82		SW	1	Fine.
	66	2	0	64	60	29,78	66		SW	1	Fair.

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1805	Six's Therm. least and greatest Heat.	Time. H. M.	Therm. without. A. M.	Therm. within. P. M.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
	°		°	°		°				
May 17	51	7 0	52	59	29,90	81		W	1	Cloudy.
	60	2 0	59	60	29,96	76		N	1	Cloudy.
18	50	7 0	52	59	30,04	82		NE	1	Cloudy.
	60	2 0	59	61	30,04	76		NE	1	Cloudy.
19	50	7 0	52	60	29,98	88	0,145	ENE	1	Cloudy.
	62	2 0	62	61	29,98	77		SE	1	Cloudy.
20	50	7 0	52	60	30,06	86		E	1	Cloudy.
	62	2 0	61	61	30,07	78		E	1	Cloudy.
21	49	7 0	51	60	30,12	86		E	1	Cloudy.
	65	2 0	65	62	30,08	77		ENE	1	Fair.
22	50	7 0	53	61	29,88	85		NE	1	Cloudy.
	67	2 0	64	64	29,83	77		NE	1	Fair.
23	45	7 0	48	61	29,83	75		NE	2	Cloudy.
	58	2 0	53	63	29,86	68		NE	2	Fair.
24	41	7 0	47	60	30,05	78		NE	2	Fair.
	59	2 0	57	62	30,04	69		N	1	Fair.
25	49	7 0	52	61	30,02	78		NE	1	Fair.
	63	2 0	63	62	30,02	71		E	1	Cloudy.
26	44	7 0	48	60	30,02	80		E	1	Fine.
	59	2 0	59	61	30,00	71		E	1	Fine.
27	44	7 0	51	60	30,02	83		NE	1	Fine.
	63	2 0	63	61	30,06	71		E	1	Fine.
28	44	7 0	51	60	30 21	83		NE	1	Hazy.
	60	2 0	60	62	30,22	75		E	1	Fine.
29	44	7 0	49	60	30,26	82		E	1	Fine.
	67	2 0	66	61	30,24	65		E	1	Fine.
30	50	7 0	55	60	30,16	78		SW	1	Hazy.
	72	2 0	72	62	30,15	70		NE	1	Fair.
31	47	7 0	52	61	30,24	78		NE	1	Fair.
	63	2 0	62	62	30,30	69		NE	1	Fine.

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1805	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. A. M.	Therm. within. P. M	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
June 1	45°	7	0	49°	61°	30,37	80°		NE	1	Fair.
	60	2	0	58	61	30,37	70		NE	1	Cloudy.
2	41	7	0	47	60	30,36	81		NE	1	Fine.
	60	2	0	60	61	30,32	71		NE	2	Fair.
3	44	7	0	48	60	30,22	81		W	1	Fine.
	71	2	0	71	62	30,08	70		NW	2	Fine.
4	52	7	0	53	61	30,03	83		NE	1	Cloudy.
	60	2	0	58	61	30,04	73		NE	1	Cloudy.
5	47	7	0	50	60	30,05	72		ENE	1	Cloudy.
	58	2	0	56	60	30,02	68		E	1	Cloudy.
6	48	7	0	50	59	29,89	76		S	1	Fine.
	67	2	0	64	60	29,85	67		W	1	Cloudy.
7	47	7	0	48	59	30,05	77		SW	1	Fine.
	69	2	0	67	61	30,07	67		S	1	Fair.
8	51	7	0	53	60	30,10	84		SW	2	Fair.
	71	2	0	69	61	30,08	68		SSW	2	Fair.
9	56	7	0	59	61	29,89	80		E	1	Cloudy.
	74	2	0	70	63	29,84	78		SSE	2	Cloudy.
10	59	7	0	60	62	29,66	90	0,185	SSE	1	Rain.
	66	2	0	64	63	29,54	88		S	2	Cloudy.
11	56	7	0	59	62	29,43	87	0,215	S	2	Cloudy.
	68	2	0	63	63	29,49	80		S	2	Rain.
12	49	7	0	52	61	29,91	82	0,388	S	2	Fine.
	66	2	0	58	62	30,03	78		S	2	Rain.
13	48	7	0	50	62	30,22	88	0,095	S	1	Fine.
	67	2	0	66	62	30,21	73		S	1	Cloudy.
14	55	7	0	55	62	30,08	87	0,075	S	1	Rain.
	56	2	0	54	62	29,90	88		N	1	Rain.
15	49	7	0	51	61	29,82	92	1,190	N	2	Rain.
	62	2	0	61	62	29,96	70		N	1	Fair.
16	49	7	0	52	61	30,08	82		W	1	Fine.
	66	2	0	66	62	30,06	68		W	1	Cloudy.

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		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
June 17	54°	7	0	56°	61°	30,02	83°		W	1	Cloudy.
	68	2	0	67	62	29,97	67		WNW	1	Cloudy.
18	54	7	0	57	61	29,76	86	0,068	WNW	2	Fair.
	68	2	0	68	62	29,77	68		NW	2	Fair.
19	50	7	0	53	61	30,00	80		NW	1	Fair.
	66	2	0	61	62	30,04	75		N	1	Rain.
20	49	7	0	51	61	29,98	83	0,160	NE	1	Cloudy.
	58	2	0	56	62	30,02	76		NE	1	Cloudy.
21	49	7	0	50	60	30,12	85	0,175	NE	1	Cloudy.
	57	2	0	55	60	30,12	77		NE	1	Cloudy.
22	49	7	0	50	59	30,16	80		NE	1	Cloudy.
	60	2	0	58	60	30,17	73		NE	1	Cloudy.
23	50	7	0	52	60	30,22	76		NE	1	Cloudy.
	65	2	0	63	60	30,20	73		NE	1	Cloudy.
24	51	7	0	55	60	30,00	84		W	1	Hazy.
	75	2	0	74	62	29,87	69		NW	2	Fair.
25	56	7	0	59	61	29,70	70		W	1	Cloudy.
	64	2	0	63	62	29,64	82		WNW	1	Rain.
26	46	7	0	49	61	29,87	80	0,085	W	1	Fair.
	66	2	0	65	62	29,91	68		SW	1	Fair.
27	49	7	0	52	61	29,89	77		SSW	2	Fair.
	67	2	0	65	62	29,85	72		SW	2	Cloudy.
28	54	7	0	55	61	29,82	87	0,200	SSW	1	Cloudy.
	66	2	0	60	62	29,82	83		N	1	Rain.
29	49	7	0	52	61	30,01	85	0,478	NE	1	Fine.
	64	2	0	62	62	30,09	70		NE	1	Hazy.
30	50	7	0	52	61	30,20	85		E	1	Fine.
	71	2	0	68	63	30,18	69		W	1	Fair.

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1805	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. A.M.	Therm. within. P. M.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°			°	°		°				
July 1	52	7	0	56	62	30,26	81		N	1	Fine.
	71	2	0	69	63	30,27	70		SW	1	Fair.
2	58	7	0	58	62	30,16	78		SSW	1	Cloudy.
	68	2	0	66	63	30,05	75		SW	1	Cloudy.
3	60	7	0	58	62	29,93	87		SW	1	Cloudy.
	67	2	0	67	63	29,87	85		SSW	1	Rain.
4	57	7	0	59	63	29,74	83	0,020	SSW	1	Fair.
	79	2	0	78	66	29,67	69		SSE	2	Cloudy.
5	62	7	0	62	65	29,58	82		S	2	Cloudy.
	70	2	0	68	65	29,64	73		S	2	Cloudy.
6	55	7	0	56	65	29,76	85	0,420	SW	2	Fair.
	69	2	0	67	65	29,78	75		SW	2	Rain.
7	51	7	0	54	64	29,93	86	0,380	WSW	2	Fair.
	70	2	0	67	65	29,96	75		SW	1	Cloudy.
8	57	7	0	58	64	29,98	88	0,035	SSW	1	Cloudy.
	62	2	0	61	64	29,97	85		SSW	1	Cloudy.
9	52	7	0	56	64	30,08	85	0,016	NE	1	Fair.
	70	2	0	69	66	30,08	71		NE	1	Fair.
10	53	7	0	54	64	30,02	85		W	1	Hazy.
	74	2	0	72	65	29,96	70		NW	1	Cloudy.
11	57	7	0	59	64	30,05	86		NE	1	Cloudy.
	64	2	0	62	65	30,08	80		NE	1	Cloudy.
12	52	7	0	56	63	30,11	80		NE	1	Cloudy.
	68	2	0	67	64	30,08	70		NE	1	Fair.
13	57	7	0	58	64	30,04	81		NNE	1	Cloudy.
	68	2	0	68	65	30,03	70		NE	1	Fair.
14	51	7	0	54	64	30,03	83		NE	1	Cloudy.
	67	2	0	66	65	30,01	77		NE	1	Cloudy.
15	52	7	0	54	64	30,04	80		NE	1	Cloudy.
	65	2	0	65	64	30,04	78		NE	1	Cloudy.
16	54	7	0	57	64	30,07	79		NE	1	Fine.
	69	2	0	68	65	30,09	70		NE	1	Fair.

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		H.	M.	A. M.	P. M.	Inches.		Inches.	Points.	Str.	
	°			°	°		°				
July 17	54	7	0	55	64	30,17	80		NE	1	Cloudy.
	63	2	0	63	64	30,17	76		NE	1	Cloudy.
18	50	7	0	53	63	30,16	80		NE	1	Cloudy.
	65	2	0	64	64	30,17	73		NE	1	Cloudy.
19	52	7	0	55	63	30,14	80		NE	1	Cloudy.
	66	2	0	65	64	30,10	75		ENE	1	Fair.
20	55	7	0	57	63	30,03	85		WSW	1	Hazy.
	76	2	0	76	65	29,98	75		SW	1	Cloudy.
21	60	7	0	62	64	29,76	93	0,565	S	2	Rain.
	69	2	0	68	65	29,59	91		S	2	Cloudy.
22	55	7	0	57	64	29,78	90	0,290	WSW	1	Cloudy.
	71	2	0	70	65	29,77	70		WSW	1	Cloudy.
23	59	7	0	60	64	29,47	92	0,230	S	2	Rain.
	71	2	0	68	65	29,31	81		S	2	Cloudy.
24	54	7	0	57	64	29,56	86	0,215	W	2	Cloudy.
	68	2	0	67	65	29,70	80		WNW	2	Fair.
25	55	7	0	57	64	29,94	80		WNW	2	Fair.
	67	2	0	65	64	30,06	75		NW	2	Cloudy.
26	52	7	0	53	64	30,14	85		WSW	1	Fine.
	73	2	0	73	65	30,07	70		SW	1	Fair.
27	57	7	0	61	64	29,80	81		SE	1	Cloudy.
	74	2	0	73	66	29,71	73		S	1	Hazy.
28	59	7	0	61	65	29,73	86		S	2	Cloudy.
	70	2	0	69	66	29,72	76		S	2	Fair.
29	57	7	0	60	65	29,76	85		S	2	Cloudy.
	69	2	0	69	66	29,78	71		S	2	Fair.
30	55	7	0	57	65	29,83	87		S	1	Fine.
	69	2	0	69	66	29,84	72		S	2	Fair.
31	54	7	0	56	65	29,81	86		SSE	2	Cloudy.
	70	2	0	67	66	29,72	78		SSW	1	Cloudy.

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1805	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. A. M.	Therm. within. P. M.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°			°	°		°				
Aug. 1	55	7	0	57	65	29,66	87	0,070	SSW	1	Cloudy.
	70	2	0	69	65	29,63	77		SSW	1	Rain.
2	58	7	0	59	65	29,51	87	0,315	SE	2	Rain.
	69	2	0	67	66	29,49	82		S	2	Cloudy.
3	58	7	0	60	65	29,75	90	1,055	W	2	Cloudy.
	72	2	0	72	66	29,82	72		W	1	Hazy.
4	52	7	0	54	65	29,95	90		SW	1	Hazy.
	73	2	0	71	66	29,94	73		S	2	Cloudy.
5	61	7	0	63	65	29,56	88		SW	2	Cloudy. { Much wind last night.
	73	2	0	73	67	29,72	67		WSW	2	Fair.
6	55	7	0	57	66	29,76	84		SW	2	Fine.
	73	2	0	72	67	29,80	67		WSW	2	Fair.
7	52	7	0	54	66	29,94	83		SW	1	Fine.
	71	2	0	69	67	29,92	70		S	2	Cloudy.
8	57	7	0	58	65	29,90	86	0,225	SE	1	Cloudy.
	73	2	0	73	67	29,92	72		S	1	Cloudy.
9	58	7	0	60	66	29,92	90	0,275	S	2	Cloudy.
	75	2	0	74	67	29,95	78		S	2	Cloudy.
10	61	7	0	63	66	30,08	87		SW	1	Cloudy.
	74	2	0	73	68	30,10	71		W	1	Fair.
11	58	7	0	60	67	30,12	84		NE	1	Cloudy.
	76	2	0	75	69	30,05	72		S	1	Fine.
12	58	7	0	62	68	29,88	80		NE	1	Fair.
	79	2	0	78	71	29,80	71		SSE	1	Fair.
13	61	7	0	61	69	29,84	85		NE	1	Cloudy.
	71	2	0	70	70	29,97	70		WNW	1	Fair.
14	60	7	0	60	68	30,00	87		W	1	Fair.
	70	2	0	70	69	30,02	70		W	1	Fair.
15	51	7	0	53	65	30,11	84	0,112	W	1	Fine.
	71	2	0	71	68	30,07	67		WNW	1	Cloudy.
16	57	7	0	58	66	30,00	83	0,020	W	1	Cloudy.
	73	2	0	71	67	29,96	72		W	1	Cloudy.

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1805	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Aug. 17	60	7	0	60	67	30,00	90	0,068	NE	1	Cloudy.
	72	2	0	71	68	30,05	72		NE	1	Fine.
18	60	7	0	61	67	29,97	88		W	1	Cloudy.
	73	2	0	71	68	29,88	74		S	2	Cloudy.
19	56	7	0	57	66	29,63	90	0,740	WNW	1	Fair.
	68	2	0	63	67	29,59	80		NW	1	Rain.
20	57	7	0	57	66	29,60	92	0,565	NW	1	Rain.
	68	2	0	68	67	29,70	81		NE	1	Fair.
21	59	7	0	59	65	29,94	91	0,095	NNE	1	Cloudy.
	62	2	0	61	66	30,05	83		NNE	1	Cloudy.
22	57	7	0	58	65	30,20	83		NNW	1	Cloudy.
	68	2	0	66	66	30,22	77		WNW	1	Cloudy.
23	61	7	0	61	66	30,24	92		W	1	Cloudy.
	73	2	0	73	68	30,27	73		WNW	1	Fair.
24	62	7	0	62	66	30,24	85		W	1	Cloudy.
	73	2	0	73	68	30,24	72		NW	1	Fair.
25	58	7	0	59	65	30,12	91		W	1	Cloudy.
	75	2	0	74	68	30,04	73		WSW	1	Fair.
26	59	7	0	61	67	29,98	88		SW	1	Cloudy.
	71	2	0	68	67	29,93	81		SW	1	Cloudy.
27	57	7	0	59	66	29,93	88		SW	2	Fair.
	74	2	0	73	68	29,94	72		W	2	Fair.
28	59	7	0	59	66	29,96	87		SW	2	Cloudy.
	71	2	0	70	67	29,55	78		SW	2	Cloudy.
29	62	7	0	63	66	29,98	88		SW	1	Fair.
	76	2	0	75	69	30,05	75		SW	1	Fair.
30	61	7	0	63	67	30,10	84		NE	1	Cloudy.
	73	2	0	73	69	29,95	78		S	2	Fair.
31	59	7	0	61	67	29,80	87		SW	1	Cloudy.
	71	2	0	69	68	29,70	78		SSW	2	Cloudy.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sept. 1	° 58	7	0	59	66	29,63	° 88		SW	1	Fair.
	72	2	0	72	68	29,67	73		WNW	2	Fair.
2	57	7	0	58	67	29,93	86	0,135	NE	1	Cloudy.
	67	2	0	66	67	29,96	74		NW	1	Cloudy.
3	57	7	0	58	67	29,85	86		SW	1	Cloudy.
	70	2	0	69	67	29,92	76		SW	2	Cloudy.
4	58	7	0	59	66	29,73	90	0,060	SW	1	Cloudy.
	72	2	0	72	67	29,70	75		SSW	1	Fair.
5	61	7	0	62	66	29,60	87	0,022	S	2	Cloudy.
	73	2	0	73	68	29,68	76		S	2	Fair.
6	58	7	0	60	67	29,60	93	0,702	S	1	Rain. [Lightning and thunder.]
	71	2	0	70	68	29,48	74		S	2	Cloudy. [At times wind very high.]
7	60	7	0	60	66	29,58	83	0,110	S	2	Cloudy.
	67	2	0	66	66	29,33	80		S	2	Cloudy.
8	57	7	0	58	65	29,52	88	0,075	SW	2	Cloudy.
	68	2	0	67	66	29,63	73		SW	2	Fair.
9	52	7	0	53	65	29,93	88		SW	1	Fine.
	69	2	0	69	66	30,01	74		WSW	1	Fair.
10	52	7	0	53	64	30,16	84		SW	1	Fair.
	71	2	0	70	67	30,12	67		SW	1	Fine.
11	62	7	0	62	66	30,06	93		SW	1	Cloudy.
	73	2	0	71	67	30,05	79		SW	1	Cloudy.
12	61	7	0	61	67	30,04	92		S	1	Cloudy.
	72	2	0	70	68	30,04	79		NW	1	Cloudy.
13	53	7	0	54	66	30,10	87	0,015	WSW	1	Fair.
	70	2	0	69	66	30,09	76		W	1	Cloudy.
14	56	7	0	57	66	30,16	88		W	1	Fine.
	73	2	0	72	68	30,18	74		SW	1	Fine.
15	54	7	0	55	66	30,22	87		W	1	Fine.
	72	2	0	70	68	30,15	81		S	1	Fine.
16	56	7	0	56	67	30,02	88		S	1	Hazy.
	75	2	0	75	69	29,93	77		S	2	Fine.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sep. 17	°57	7	0	60	68	29,96	°90		S	1	Cloudy.
	72	2	0	72	69	29,94	78		S	2	Fair.
18	58	7	0	59	68	30,00	91		S	1	Foggy.
	79	2	0	79	71	29,95	76		SE	1	Fine.
19	64	7	0	65	69	29,83	85		E	1	Fine.
	71	2	0	71	70	29,86	84		SE	1	Fair.
20	57	7	0	57	69	29,98	83	0,360	WSW	2	Fair. Last night there was much lightning & thunder.
	68	2	0	67	69	30,15	68		WNW	2	Fair.
21	57	7	0	57	67	29,92	83		ESE	2	Cloudy.
	66	2	0	63	68	29,80	78		S	2	Cloudy.
22	52	7	0	55	66	29,88	88		NE	1	Fair.
	65	2	0	64	67	29,98	83		N	1	Rain.
23	50	7	0	52	65	30,12	86	0,046	WNW	1	Cloudy.
	64	2	0	63	65	30,12	80		N	1	Cloudy.
24	46	7	0	48	63	30,14	86		SW	1	Cloudy.
	60	2	0	58	65	30,14	73		NE	1	Fair.
25	43	7	0	43	62	30,18	83		W	1	Fine.
	61	2	0	61	64	30,18	73		WNW	1	Fine.
26	53	7	0	53	62	30,16	87		W	1	Cloudy.
	64	2	0	63	63	30,16	78		NNE	1	Cloudy.
27	50	7	0	50	62	30,24	86		NE	1	Fine.
	65	2	0	64	63	30,24	80		NE	1	Fine.
28	52	7	0	54	62	30,36	90		SW	1	Cloudy.
	64	2	0	64	63	30,41	81		NE	1	Fair.
29	49	7	0	50	62	30,60	90		NE	1	Cloudy.
	62	2	0	61	63	30,61	73		NE	1	Fair.
30	46	7	0	48	61	30,56	88		NE	1	Fine.
	61	2	0	61	62	30,50	80		NE	1	Cloudy.

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1805	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Oct. 1	54	7	0	55	61	30,44	83		NE	2	Cloudy.
	61	2	0	61	61	30,38	78		NE	1	Cloudy.
2	55	7	0	56	61	30,30	89		NE	1	Cloudy.
	63	2	0	62	63	30,28	77		ENE	1	Fair.
3	52	7	0	52	62	30,23	87		E	1	Fine.
	61	2	0	61	64	30,20	76		E	1	Fine.
4	49	7	0	50	61	30,25	88		ENE	2	Fine.
	60	2	0	60	63	30,25	75		E	2	Fine.
5	46	7	0	48	61	30,31	86		NE	1	Fine.
	60	2	0	60	63	30,31	73		E	1	Fine.
6	43	7	0	43	61	30,31	85		NE	1	Fine.
	60	2	0	59	61	30,31	78		NE	1	Fine.
7	39	7	0	41	59	30,34	85		ENE	1	Fine.
	58	2	0	58	62	30,28	78		WNW	1	Fine.
8	48	7	0	51	60	30,10	90		W	1	Cloudy.
	63	2	0	63	62	30,05	87		WNW	1	Cloudy.
9	53	7	0	53	61	29,96	92		ENE	1	Cloudy.
	58	2	0	58	62	29,85.	82		E	1	Cloudy.
10	53	7	0	53	60	29,45	93	0,123	E	1	Rain.
	54	2	0	54	62	29,54	84		NE	1	Cloudy.
11	39	7	0	41	58	29,98	82	0,180	NE	2	Cloudy.
	52	2	0	51	60	30,08	77		NE	1	Cloudy.
12	36	7	0	36	57	30,09	83		SW	1	Fair.
	52	2	0	52	59	29,95	79		S	1	Fair.
13	47	7	0	48	57	29.65	83		SE	2	Cloudy.
	53	2	0	53	58	29,60	89		SW	1	Rain.
14	41	7	0	42	56	29,60	90	0,125	SW	1	Fair.
	56	2	0	56	59	29.54	84		SW	1	Fair.
15	47	7	0	47	57	29,32	92	0,380	NE	1	Rain.
	54	2	0	53	60	29,43	80		NE	1	Fair.
16	44	7	0	45	57	29,36	87	0,040	NE	1	Cloudy.
	52	2	0	52	58	29,19	83		NE	2	Cloudy.

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Oct. 17	40°	7	0	41	55	29,42	83°		WNW	2	Cloudy.
	48	2	0	47	57	29,55	85		WNW	1	Cloudy.
18	39	7	0	40	55	29,85	87	0,035	NE	1	Fine.
	51	2	0	51	57	29,95	78		NE	2	Fair.
19	38	7	0	38	55	30,15	86		NE	1	Fine.
	51	2	0	51	58	30,20	80		NE	1	Fair.
20	38	7	0	38	55	30,26	88		NE	1	Foggy.
	52	2	0	51	56	30,22	87		ENE	1	Fair.
21	45	7	0	45	55	30,18	88		NE	1	Cloudy.
	53	2	0	53	57	30,14	83		ENE	1	Cloudy.
22	49	7	0	49	55	30,04	83		E	2	Cloudy.
	52	2	0	52	58	29,93	74		E	2	Fair.
23	42	7	0	42	54	29,77	84		E	2	Fine.
	52	2	0	52	58	29,72	82		E	2	Fine.
24	45	7	0	48	55	29,66	90		E	1	Rain.
	53	2	0	53	57	29,55	90		E	1	Cloudy.
25	52	7	0	52	56	29,35	95	0,265	E	1	Cloudy.
	59	2	0	59	58	29,45	93		S	1	Cloudy.
26	51	7	0	51	57	29,46	95		E	1	Cloudy.
	54	2	0	54	58	29,34	93		E	1	Cloudy.
27	50	7	0	50	57	29,34	97	0,235	NE	1	Rain.
	51	2	0	51	58	29,43	92		NE	1	Cloudy.
28	45	7	0	45	57	29,62	83		NE	2	Cloudy.
	47	2	0	47	58	29,66	76		NE	2	Cloudy.
29	43	7	0	43	56	29,55	90		NE	2	Cloudy.
	47	2	0	47	57	29,58	83		NE	1	Cloudy.
30	37	7	0	37	54	29,84	90		NE	2	Fine.
	48	2	0	47	57	30,05	80		NE	2	Fine.
31	35	7	0	35	54	30,40	86		SW	1	Fair.
	45	2	0	45	55	30,45	86		SSW	1	Foggy.

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	°			°	°	Inches.	°	Inches.			
Nov. 1	37	7	0	37	54	30,36	83		ESE	1	Fine.
	44	2	0	44	54	30,18	71		E	1	Cloudy.
2	36	7	0	37	52	30,07	83		NE	1	Fair.
	48	2	0	48	55	30,10	78		NE	1	Fair.
3	34	7	0	34	52	30,23	86		NE	1	Fine.
	49	2	0	49	54	30,27	82		E	1	Fine.
4	37	7	0	38	52	30,34	90		E	1	Fine.
	49	2	0	49	55	30,34	84		E	1	Fine.
5	31	7	0	35	52	30,41	88				Foggy.
	40	2	0	40	54	30,38	87				Foggy.
6	38	7	0	41	52	30,38	90		SE	1	Cloudy.
	45	2	0	45	54	30,38	88		S	1	Cloudy.
7	44	7	0	45	52	30,38	93		S	1	Cloudy.
	46	2	0	46	54	30,36	94		SSE	1	Cloudy.
8	39	7	0	40	53	30,34	93		SE	1	Foggy.
	41	2	0	41	54	30,34	92		E	1	Cloudy.
9	39	7	0	42	52	30,26	92		E	1	Cloudy.
	42	2	0	42	53	30,21	91		E	1	Cloudy.
10	39	7	0	40	52	30,15	90		E	1	Cloudy.
	44	2	0	44	53	30,15	90		E	1	Cloudy.
11	41	7	0	41	52	30,28	92		E	1	Cloudy.
	44	2	0	42	53	30,34	87		E	1	Cloudy.
12	40	7	0	40	51	30,35	90		E	2	Cloudy.
	43	2	0	43	52	30,28	89		ESE	1	Cloudy.
13	38	7	0	40	51	30,35	93	0,455	ENE	1	Cloudy.
	46	2	0	45	52	30,42	82		NE	1	Cloudy.
14	35	7	0	38	50	30,52	90		NE	1	Cloudy.
	46	2	0	46	52	30,54	83		NE	1	Fair.
15	40	7	0	44	52	30,63	94		NE	1	Cloudy.
	49	2	0	48	53	30,68	80		NE	1	Cloudy.
16	37	7	0	40	52	30,68	90		NE	1	Cloudy.
	47	2	0	47	54	30,68	80		NE	1	Cloudy.

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	°					°				
Nov. 17	36	7 0	36	51	30,59	91		ENE	1	Fine.
	44	2 0	44	54	30,50	80		E	1	Fine.
18	33	7 0	33	50	30,28	83		E	1	Fine.
	42	2 0	42	53	30,18	79		E	1	Fair.
19	31	7 0	33	50	30,08	87		W	1	Cloudy.
	41	2 0	38	52	30,05	88		SW	1	Cloudy.
20	36	7 0	40	50	30,20	93		ENE	1	Cloudy.
	45	2 0	44	53	30,28	90		ENE	1	Fair.
21	32	7 0	32	49	30,46	85		N	1	Fine.
	39	2 0	39	51	30,41	81		WSW	1	Fine.
22	33	7 0	37	48	30,23	90		SW	1	Cloudy.
	47	2 0	47	52	30,16	85		SW	1	Cloudy.
23	37	7 0	37	49	30,30	89		N	1	Cloudy.
	43	2 0	43	53	30,30	83		N	1	Fair.
24	32	7 0	34	49	30,28	88		SW	1	Cloudy.
	44	2 0	43	52	30,24	90		W	1	Cloudy.
25	42	7 0	42	50	30,22	90		WSW	1	Cloudy.
	47	2 0	47	53	30,17	85		WSW	1	Cloudy.
26	42	7 0	42	50	30,22	90		SW	1	Cloudy.
	47	2 0	47	53	30,21	81		WSW	1	Cloudy.
27	42	7 0	42	51	30,20	90		E	1	Cloudy.
	46	2 0	46	53	30,14	83		E	1	Cloudy.
28	37	7 0	38	51	30,01	87		E	1	Cloudy.
	50	2 0	45	53	29,90	87		E	1	Cloudy.
29	48	7 0	44	52	29,55	94		S	1	Cloudy.
	54	2 0	54	55	29,50	93		S	1	Cloudy.
30	53	7 0	54	53	29,28	97	0,340	SSW	2	Cloudy.
	55	2 0	54	57	29,21	94		S	2	Rain.

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1805	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Dec. 1	43	8	0	43	55	29,12	90	0,060	SW	1	Cloudy.
	46	2	0	46	56	29,15	87		S	1	Cloudy.
2	34	8	0	34	53	29,60	84	0,062	NNW	2	Cloudy.
	38	2	0	38	55	29,76	81		NNW	2	Fair.
3	32	8	0	37	52	30,02	90		W	2	Cloudy.
	44	2	0	43	54	29,94	87		SW	1	Rain.
4	43	8	0	46	53	30,00	96	0,175	SW	1	Cloudy.
	52	2	0	52	55	30,03	96		SW	1	Cloudy.
5	43	8	0	43	54	30,25	91		W	1	Fine.
	49	2	0	49	56	30,27	86		W	1	Fine.
6	43	8	0	46	54	30,22	96		SW	1	Fair.
	53	2	0	52	57	30,14	94		SW	1	Cloudy.
7	49	8	0	51	55	29,93	94		S	2	Cloudy.
	53	2	0	53	57	29,87	94		SW	2	Cloudy.
8	43	8	0	44	54	29,81	90	0,016	SW	1	Cloudy.
	48	2	0	48	57	29,77	83		SW	2	Cloudy.
9	43	8	0	47	54	29,38	90		S	2	Cloudy.
	46	2	0	43	56	29,17	91		NW	2	Rain.
10	35	8	0	38	53	29,15	92	0,725	WSW	1	Rain.
	41	2	0	41	56	29,25	90		NW	1	Fair.
11	31	8	0	32	52	29,44	77		NNW	1	Fine.
	39	2	0	39	54	29,56	85		NNW	1	Cloudy.
12	32	8	0	35	52	29,25	92		NNE	1	Cloudy.
	37	2	0	37	52	29,46	88		N	1	Cloudy.
13	24	8	0	24	48	29,69	80		NNW	1	Fine.
	30	2	0	29	50	29,64	80		NW	2	Fine.
14	29	8	0	30	47	29,62	83		NW	2	Fine.
	36	2	0	34	48	29,64	87		NW	1	Fair.
15	32	8	0	34	46	29,83	91		NW	1	Snow.
	37	2	0	37	48	29,83	90		NW	2	Cloudy.
16	28	8	0	28	45	30,08	85		NNE	2	Fine.
	32	2	0	32	48	30,16	81		NNE	2	Fine.

METEOROLOGICAL JOURNAL

for December, 1805.

1805	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Dec. 17	25	8	0	26	44	30,38	90		NE	2	Fair.
	32	2	0	32	47	30,41	88		NE	1	Cloudy.
18	29	8	0	33	44	30,33	90		W	1	Clou[illegible].
	39	2	0	38	47	30,29	91		W	1	Fair.
19	38	8	0	35	44	30,07	89		SW	1	Cloudy.
	44	2	0	44	47	29,94	91		S	1	Rain.
20	43	8	0	47	47	29,63	96	0,025	SSW	1	Cloudy.
	52	2	0	51	49	29,48	96		SSW	1	Rain.
21	50	8	0	51	50	29,22	95		SW	2	Cloudy.
	53	2	0	50	53	29,00	93		SSW	2	Rain.
22	41	8	0	41	50	28,85	95	0,375	S	1	Fair.
	46	2	0	46	53	28,81	90		S	1	Fair.
23	35	8	0	36	49	29,00	88		SW	1	Cloudy.
	40	2	0	40	52	29,12	87		S	1	Fair.
24	30	8	0	37	50	29,52	94		N	1	Cloudy.
	40	2	0	39	52	29,70	91		NE	1	Fair.
25	32	8	0	34	47	29,83	90		SW	1	Cloudy.
	46	2	0	42	51	29,68	90		S	1	Rain.
26	42	8	0	45	48	28,96	95	0,310	S	2	Rain. [Much wind last night.
	48	2	0	48	52	28,87	96		S	2	Cloudy.
27	37	8	0	37	48	29,60	90		NE	2	Fine.
	40	2	0	40	52	29,83	88		NE	2	Fine.
28	31	8	0	35	49	29,98	92		SSE	1	Fair.
	42	2	0	42	51	29,81	93		S	2	Rain.
29	42	8	0	46	50	29,87	97	0,035	SSW	1	Cloudy.
	53	2	0	53	52	29,81	97		SW	1	Cloudy.
30	51	8	0	51	52	29,87	96	0,016	SW	2	Rain.
	54	2	0	53	53	29,90	96		SW	1	Cloudy.
31	53	8	0	53	53	29,94	97		S	2	Cloudy.
	54	2	0	54	55	29,82	96		S	2	Cloudy.

1805.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	48	25	36,2	48	26	36,9	54	44	49,4	30,41	28,88	29,61	95	79	90,6	1,513
February	54	23	40,7	54	23	40,8	58	45	51,8	30,22	28,86	29,82	95	75	85,9	1,045
March	62	29	44,0	62	30	44,3	60	51	55,5	30,24	29,40	29,96	93	69	80,8	0,882
April	64	35	48,0	63	35	48,2	61	54	57,2	30,38	29,31	29,82	92	64	78,7	1,583
May	72	37	52,4	64	39	53,4	64	55	59,2	30,26	29,33	29,90	89	65	77,7	0,851
June	75	41	57,7	74	47	57,8	63	59	61,2	30,37	29,43	29,99	92	67	77,7	3,314
July	79	50	62,1	78	53	62,5	66	62	64,3	30,27	29,31	29,92	92	69	79,6	2,171
August	79	51	65,0	78	53	65,0	71	65	66,8	30,27	29,49	29,92	92	67	80,6	3,535
September	79	43	62,0	79	43	61,8	71	65	66,0	30,61	29,33	30,00	93	67	81,9	1,525
October	63	35	49,6	63	35	49,8	64	54	58,3	30,45	29,19	29,89	97	73	84,9	1,383
November	55	31	41,8	54	32	42,1	57	48	52,2	30,68	29,21	30,24	97	71	87,5	0,795
December	54	25	40,8	54	26	41,4	57	44	51,2	30,41	28,81	29,69	97	80	90,3	1,799
Whole year			50,0			50,5			57,8			29,90			83,0	20,396

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

Variation of the Magnetic Needle.

1805.

June - - - -	24° 7',8 W.
Dip of the needle - -	70 21.

Note. Subtract 2' from the variation of last year, for the error of the instrument.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCVI.

PART II.

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MDCCCVI.

CONTENTS.

PHILOSOPHICAL

TRANSACTIONS.

X. *Observations upon the Marine Barometer, made during the Examination of the Coasts of New Holland and New South Wales, in the Years* 1801, 1802, *and* 1803. *By* Matthew Flinders, *Esq. Commander of his Majesty's Ship* Investigator. *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S. &c. &c. &c.*

Read March 27, 1806.

Isle of France, Aug. 19, 1805.

A FORE-KNOWLEDGE of the wind and weather is an object so very interesting to all classes of men, and the changes in the mercurial barometer affording the means which appear most conducive to it, a system that should with certainty explain the connection between the variations of the mercury and those in the atmosphere under all circumstances, becomes greatly desirable; to seamen, more especially, whose safety and success depend so much upon being duly prepared for changes of wind, and the approaching storm, it would be an

acquisition of the first importance: in a more extended view, I may say, that the patriot and the philanthropist must join with the philosopher and the mariner in desiring its completion. So long and widely-extended a course of observation, however, seems requisite to form even a basis for it, that a complete system is rather the object of anxious hope than of reasonable expectation. Much has been done towards it, but so much appears to remain, that any addition to the common stock, however small, or though devoid of philosophical accuracy, I have thought would be received by the learned with candour. With this prepossession, I venture to submit to them some observations upon the movement and state of the mercury upon the coasts of New Holland and New South Wales, the Terra Australis, or Australia, of the earlier charts.

The principal circumstance that has led me to think these observations worth some attention, is the coincidence that took place between the rising and falling of the mercury, and the setting in of winds that blew from the sea and from off the land, to which there seemed to be at least as much reference, as to the strength of the wind or state of the atmosphere; a circumstance that I do not know to have been before noticed. The immediate relation of the most material of these facts, it is probable, will be more acceptable than any prefatory hypothesis of mine; and to it, therefore, I proceed; only premising, that a reference to the chart of Australia will be necessary to the proper understanding of some of the examples.

My examination of the shores of this extensive country began at Cape Leuwen, and continued eastward along the south coast. In King GEORGE's Sound, December 20, 1801,

after a gale from WSW, the mercury had risen from 29,42 to 29.84, and was nearly stationary for two days, the wind being then moderate at NW, with cloudy weather. On the 22d, the wind shifted to SW, blew fresh, and heavy showers of rain occasionally fell; but the mercury rose to 30,02, and remained at that height for thirty hours; and on the weather clearing up, and the wind becoming moderate in *the same quarter*, it rose to 30,28.

Fresh breezes from E and SE caused a rise in the barometer in King George's Sound, once to 30,20, and a second time to 30,18, although the weather at these times was hazy; but with light winds from the same direction, which were probably local sea breezes only, the mercury stood about 29,95 in that neighbourhood.

2d Example. Jan. 12, 1802, in D'Entrecasteaux's Archipelago, the mercury rose to 30, 23, previously to a fresh breeze setting in from the eastward. In the evening of the 13th it blew strong from ESE, with hazy weather, and a rapid fall of the mercury to 29,94 had then taken place; but instead of the wind increasing, or bad weather coming on, the wind died away suddenly, and a light breeze came off the land at midnight, with cloudy weather.

At the Cape of Good Hope, which is nearly in the same latitude, the mercury rises with the fresh gales that blow there from the SE in the summer season. The weather that accompanies these south-east winds, is nearly similar in both places; the atmosphere being without clouds, but containing a whitish haze, and the air usually so dry as sensibly to affect the skin, particularly of the lips.

3d. Jan. 22. Three degrees east of the Archipelago, the

mercury fell with some rapidity down to 29,65 with the wind from ESE. It was eight o'clock at night, and we prepared for a gale from that quarter; but at ten, the wind suddenly shifted to WNW, coming very light off the land. On its veering gradually round to SSW, clear of the land, at noon, 23d, it freshened, and the weather became thick; yet the mercury had then risen to 29,84, and at eight in the evening to 29,95, though the wind then blew strong. It continued to rise to 30,16 as the wind shifted round to SE, and fine weather came on; but on the wind passing round to ENE and NNE, which was off the land, the mercury fell back to 29,73, though the weather was fine and the wind moderate. On a sudden shift of wind to the SW, a fresh breeze with hazy weather, it again began to ascend, and a similar routine of wind, producing nearly the same effects upon the barometer, again took place. The effect of sea winds in raising the mercury, in opposition to a strong breeze, and of land winds in depressing it, though they were light, was here exemplified in two remarkable instances.

4th. In the neighbourhood of the Isle of St. FRANCIS of Nuyts, longitude 133½° east of Greenwich, we experienced a considerable change in the barometer. For nine days in January and February the wind continued to blow constantly, though moderately, from the eastward, mostly from the SE. It appeared like a regular trade-wind or monsoon, but so far partook of the nature of sea and land breezes, as commonly to shift more to the southward in the day, and to blow more from east and NE in the night. The weather was very hazy during these nine days; so much so, that for six of them no observation of the sun's altitude, worthy of confidence, could

be taken from the sea horizon, although the sun was sufficiently clear; and in the whole time, the mercury never once stood so high as 30 inches, but was frequently below 29,70. I considered this to be the more extraordinary, as settled winds from the eastward, and especially from SE, had before made it rise and stand high upon this coast, almost universally, even when there was a considerable degree of haze. The direction of the south coast, beyond the Isle of St. Francis, and even abreast of it, was at that time unknown to me; but I then suspected, from this change in the barometer, that we should find the shore trending to the southward, which proved to be the case. The easterly winds, then, whilst they came off the sea, caused the mercury to rise upon the south coast; but in this instance that they came from off the land, they produced a contrary effect; but it is to be observed, that the most hazy part of the time, and that during which the mercury stood lowest, was two days that the wind kept almost constantly on the north side of west, more directly off the land: its height was then between 29,65 and 29,60.

The haze did not immediately clear away on the wind shifting to the westward; notwithstanding which, and that the new wind rose to a strong breeze, and was accompanied with squalls of rain, the mercury began to ascend, and had reached 29,95 when the squalls of wind and rain were strongest; the direction of the wind being then from SSW. On its becoming moderate, between SSW and SSE, the mercury ascended to 30,14, and remained there as long as the wind was southwardly.

5th. Going up the largest of the two inlets on the south coast, in March, we were favoured with fine fresh breezes

from SSW to SSE, sometimes with fine, sometimes with dull weather, the mercury rising gradually from 30,08 to 30,22. In twenty-four hours afterwards, it fell below 30 inches, and a light breeze came from the northward, off the land, with finer weather than before. The mercury continued to fall to 29,56, where it stopped; the wind having then ceased to blow steadily from the northward, and become variable. In twenty-four hours more, the wind set in again to blow fresh from the southward, the mercury having then returned to 29.94, and it was presently up to 30,22 and 30,28. It kept nearly at this height for several days that the southwardly wind blew fresh, but on its becoming lighter, and less steady in its direction, the mercury descended; and in the calm which followed, it had fallen to 29,90. This example affords clear proof of a fresh wind from the sea making the mercury rise, whilst a light wind off the land, with finer weather, caused it to descend.

6th. The calm was the prelude to a fresh gale; but the mercury began to rise at eight in the evening when it had just sprung up; by the next noon it was at 30,10 when the wind blew strongest, and in the evening at 30,22. This gale began as gales usually, if not always, do upon this coast, in the north-west quarter, and shifted round to SW and SSW; but quicker than I have generally seen them: there was no rain with it, nor was the atmosphere either very hazy or cloudy.* The mercury continued to rise till it had reached 30,25, and then was stationary as long as the wind remained between south and west; but on its veering round to the

* I afterwards learned from Captain BAUDIN, that this gale was much heavier in Bass' Strait than we felt it at Kanguroo Island.

eastward of south, a second rise took place, and for forty hours the mercury stood as high as 30,45, the wind being then between SE by S and east: the weather was very dull and hazy during the first half of these forty hours, but finer afterwards. As the winds between SE by S and east slanted off the main land, this example seems to be in opposition to the 4th, and leads me to think, that it might have been the very extraordinary kind of haze, and perhaps some peculiarity in the interior part of the land abreast of the Isle of St. FRANCIS that in part occasioned the fall of the mercury with south-east winds; as much, perhaps, as the circumstance of the wind coming from off the shore.

After this rise in the mercury to 30,45, it fell gradually; but, for thirteen days, kept above 30 inches, the winds being generally between SE and SW, but light and variable, and the weather mostly fine.

7th. North-eastwardly winds, off the land, were the next that prevailed; they were light, and accompanied with cloudy weather and spitting rain. The mercury fell to 29,70, and remained there till the wind shifted to the west and southward, when it began to rise, and in two days was up to 30,42. At that time we were off the projection marked Π in the chart, in 139½° east longitude; the wind had then veered to the south-eastward along the shore, with a steady breeze, and the mercury remained nearly stationary so long as it lasted; but on the wind dying off, and flawing from one side and the other, it descended quickly to 30 inches. A breeze then sprung up at NW, which, within twenty-four hours, shifted suddenly to SW, and blew a gale which had near proved fatal to us. It was accompanied with rain and very

thick weather, and lasted two days; by which time, the mercury had descended to 29,58.

8th. In Bass' Strait, for several days in the month of April, the mercury stood above 30,40 with the wind from the south and eastward, sometimes blowing fresh: the weather generally fine. It then fell half an inch in eight hours, and a wind set in soon after from the north-westward which continued four days, blowing moderately, with cloudy weather, and sometimes a shower of rain; the mercury remaining stationary between 29,83 and 29,89. On this second wind dying away, a strong breeze sprung up which fixed at WSW with squally weather; but for three days no alteration took place in the barometer, until the wind shifted to NW and north, and the mercury then descended to 29,52, though the weather was finer, and wind more moderate than before.

9th. Passing along the south coast of Australia the second time, we experienced light winds from the sea for forty hours in D'ENTRECASTEAUX'S Archipelago, in the month of May: they were variable between WSW and SSE with dull cloudy weather, and the mercury stood very high, being up to 30,50 most of the time. The wind then came round to N by E and NNW; previously to which, the mercury began to descend, and it kept falling for two days till it reached 30,19, though the weather was not so cloudy as before, and the wind was equally light. On the wind veering to west and WSW the mercury rose to 30,25; but it now came on to blow fresh, with squally thick weather, yet the mercury continued nearly stationary for twenty-four hours, appearing to be kept up in consequence of the wind having shifted round to SSW, more directly from off the sea. On its increasing to a

gale, however, there was a pretty rapid descent in the barometer to 29,96; but the ascent again was equally rapid, and to a greater height, on the wind becoming moderate. In a short calm that succeeded, the mercury stood at 30,42, but on the wind setting in from the north, which was from off the land, it fell to 30,25, and remained there two days: we had then reached Bass' Strait.

From these examples upon the south coast, it appears, generally, that a change of wind from the northern, to any point in the southern half of the compass, caused the mercury to rise, and a contrary change to fall; and that the mercury stood considerably higher when the wind was from the south side of east and west, than, in similar weather, it did when the wind came from the north side; but, until it is known what are the winds that occasioned the mercury to ascend, and what to descend, upon the other coasts of Australia, it will probably be not agreed, whether it rose in consequence of the south winds bringing in a more dense air from the polar regions, and fell on its being displaced by that which came from the Tropic;—or whether the rise and higher standard of the mercury was wholly, or in part, occasioned by the first being sea winds, and the descent because those from the northward came from off the land.

The height, at which the mercury generally stood upon the south coast, seems to deserve some attention. It was very seldom down to 29,40, and only once to 29,42. Of one hundred and sixty days, from the beginning of December to May, it was nearly one-third of the time above 30 inches; and the second time of passing along the coast, from the 15th of May to the 1st of June, it only once descended to 29,96,

and that for a few hours only, its average standard for these sixteen days being 30,25. Upon the eastern half of the coast, beyond Cape Catastrophé, in March, April, and May, the mercury stood higher than it did on the western half in December, January, and February: the average standard of the first was 30,09, but that of the latter only 29,94. At the Cape of Good Hope, the mean height in the barometer, during eighteen days in October and November, was 30,07.

The marine barometer on board the Investigator, supplied to the astronomer by the Board of Longitude, was made by NAIRNE and BLUNT, and had, I believe, been employed in one or more of the voyages of Captain COOK, and perhaps in that of Captain VANCOUVER. I suspect, that it was not suspended so exactly in the proper place, as the later instruments of these makers probably are; on which account, the motion of the ship caused the mercury to stand too high; and perhaps one or two-tenths of an inch might be deducted with advantage from the heights taken at sea, but I think not when the ship was lying steadily at anchor in harbour. The barometer stood in my cabin, and the height of the mercury was taken at day-break, at noon, and at eight in the evening, by the officer of the watch; as was also that of the thermometer.

The general effects produced upon the barometer by the sea and land winds, on the east coast of Australia, will be learned from the following abridgment of our meteorological journal.

1st. In the run from Cape HOWE, in $37\frac{1}{2}°$ south latitude, to Port Jackson, in 34°, once in the month of May, and once in June, I found that the mercury descended with light winds from north, NW, west, and WSW; whilst during fresh

breezes from south and SW it ascended, and stood considerably above 30 inches; with the wind at NE and NNE it also kept above 30 inches, but not so high, nor did it rise so fast, as when the wind was from SSW. From between south and east, the winds did not blow during these times. This example does not differ so much from those on the south coast as to be decisive of any thing.

2d. The observations made during a stay of ten weeks at Port Jackson, in May, June, and July, 1802, are more in point than almost any other. Strong eastwardly winds were very prevalent at that time, and were almost always accompanied with rain and squalls; yet this weather was foretold and accompanied by a rise in the barometer, and the general height of the mercury during their continuance was 30,20: higher if the wind was on the south side of ESE, and lower if on the north side of east. The winds from south and SSW, which blow along the shore, kept the mercury up to about 30,10, when they were attended with fine weather, as they generally were; but if the weather was squally, with rain, it stood about 29,95. During settled winds from between WNW and SW, with fine weather, the mercury generally stood very low, down at 29,60;* and what is more extraordinary, when these winds were less settled, and the weather dull, with rain occasionally falling, the range of the mercury was usually between 29,80 and 30,10; nearly the same as

* My friend Colonel PATERSON, F. R. S. commander of the troops at Port Jackson, in judging of the approaching weather by the rise and fall in his barometer in the winter season, told me, that he had adopted a rule directly the reverse of the common scale. When the mercury rose high, he was seldom disappointed in his expectation of rainy, bad weather; and when it fell unusually low, he expected a continuance of fine, clear weather, with westwardly winds.

when the wind was at SSW with similar weather; the reason of which may probably be, that at some distance to the southward these westwardly winds blew more from the south, and were turned out of their course, either by the mountains, or by meeting with a north-west wind farther to the northward.

The winds from north and NW blew very seldom at this time: the mercury fell on their approach.

To the state of the mercury during our second stay at Port Jackson, in July, 1803, and part of June and August, it is not in my power to refer, as I have not been able to obtain that part of my journal from General DE CAËN.

The effects of some winds upon the barometer in this 2d example, are considerably different to what they were upon the south coast. The wind at WSW or SW with fine weather, had always caused the mercury to rise and stand high, and those from the NE to fall; whereas here, the effects of those winds were almost directly the reverse. The winds from SSW, SE, and as far as east, made it rise on both coasts, with the exception of the 4th example on the south; and from between north and WNW the mercury fell in both cases and stood low.

3d. Steering along the east coast, from Port Jackson to the northward, in July, we had the winds usually between south and SW, and sometimes WSW, the mercury being nearly stationary at 30 inches; but meeting with a spurt of the south-east trade wind in latitude 24°, we found it rise to 30,30 for two days. A westwardly wind brought it back to 30 inches for a short time; but on the trade wind finally setting in, it fixed itself between 30,20 and 30,30, as long as the wind preserved its true direction.

4th. The month of September, 1802, and the greater part of August and October, we spent upon the east coast between the latitudes 23° and 17°. The south-east trade is the regular wind here, but we had many variations. Whilst the trade prevailed, the average standard of the mercury was 30,15, and the more southwardly it was, and the fresher it blew, the higher the quicksilver rose, though it never exceeded 30,30. When the trade wind was light, it was usual for a breeze to come off the land very early in the morning, and continue till eight or nine o'clock; but these temporary land winds did not produce any alteration in the mercury, which kept at these times about 30,10. When the trade wind veered round to ENE, or more northward, which was not seldom, the mercury ranged between 30 inches and 30,10; and when a breeze from north or N by W prevailed, which was the case for a considerable part of twenty days we remained in Broad Sound, its height was something, but not much, less. These northwardly winds I take to have been the north-east wind in the offing; which had been partly turned, and in part drawn out of its direction, by the peculiar formation of this part of the east coast. There are but few instances of any steady westwardly wind prevailing; when such happened, they were generally from the north side of west; and at these times the range of mercury was between 29,95 and 30,05, which was the lowest I at any time saw it on this portion of the east coast.

The barometer was of great service to me in the investigation of this dangerous part of the east coast, where the ship was commonly surrounded with rocks, shoals, islands, or coral reefs. Near the main land, if the sea breeze was dying

off at night, and the mercury descending, I made no scruple of anchoring near the shore; knowing that it would either be a calm, or a wind would come off the land; but if the mercury kept up, I stretched off, in the expectation that it would freshen up again in a few hours. Amongst the barrier reefs, when the wind was dying away, the barometer told me, almost certainly, from what quarter it would next spring up. If the mercury stood at 30,15, or near it and was rising, I expected the proper trade wind; and if higher, that it would be well from the southward, or would blow fresh; and if it was up to 30,30, both. The falling of the mercury to 30,10 was an indication of a breeze from the north-eastward; and its descent below 30 inches that it would spring up, or shift round to the westward. This regularity of connection between the barometer and the direction of the wind, is perhaps too great to be expected at a different time of the year; and it is probable, that we should not have found it continue so strictly, had our stay amongst the barrier reefs been much prolonged.

5th. Leaving the east coast in the latitude 17° south, we steered off to the northward for Torres' Strait, in the latter part of October. As we advanced northward, I found the mercury stand gradually lower with the same kind of wind and weather. The difference was not material till we reached the latitude 13°, but afterwards, the south-east wind which had before kept the mercury up to 30,15, then permitted it to fall to 29,90; and the winds from ENE and NNE to 29,85. Was this alteration owing to the want of density in the air brought in by the south-east winds, in this lower latitude?—to our increased distance from the land?—or was it, that the

south-east wind was no longer obstructed by the coast, having a passage there through Torres' Strait?

The difference between the height of the mercury, during a north-east and a south-east wind, was much less here than before, although the weather was most unfavourable during the time of the north-east wind, and should have increased the difference in their effects. Was this owing to the general approximation to that equality which has been observed to take place in the barometer, in very low latitudes?—or that the north-east wind, still meeting with resistance from the coast, had one cause for keeping up its power, which the south-east wind had lost?

In a general summary of the winds on the east coast, those that came from between south and east caused the mercury to rise and stand highest, as they had also done upon the south-coast, with the exception of the 4th example. The winds from NE kept the mercury up above 30 inches on the east coast, and caused it to rise after all other winds except those from the south-eastward; but on the south coast, the mercury fell with them, and stood considerably below 30 inches; because, as it appears to me, they then came from off the land. During north-west winds, the mercury stood lower than at any other time upon both coasts; and on both they came from off the land.

Moderate winds from the south-westward, with fine weather, caused a descent of the mercury on the east coast; and during their continuance it was much lower than with winds from the north-eastward; but upon the south coast it rose with south-west winds, and stood much higher than when they came from the opposite quarter. For this change I cannot

see any other reason, than that the wind, which blew from the sea upon one coast, came from off the land in the other.

Although the height of the mercury upon the south coast of Australia was, upon the average, considerably above the medium standard 29,50, it was still greater upon the east coast: I cannot fix it at less than 30,08 or 30,10, whereas upon the south coast, I should take it at 30 inches; both subject to the probable error of one or two-tenths of an inch in excess. This superiority seems attributable to the greater prevalence of sea winds upon the east coast, and particularly of those from SE, which, *when all other circumstances are equal*, I have observed to raise the mercury higher than any other on this side of the equator, analogous to the effect of north-east winds in the northern hemisphere; and perhaps also, the superiority may be in part owing to the east coast having a more regular chain of higher mountains running at the back of, and parallel to it, which presents a greater obstruction to the passage of the wind over the land, than it meets on the south coast.

The greatest range of the mercury observed upon the east coast, was from 29,60 to 30,36 at Port Jackson; and within the tropic from 29,88 to 30,30; whilst upon the south coast, the range was from 29,42 to 30,51, in the western part, where the latitude very little exceeds that of Port Jackson. It is to be observed, however, that these extremes are taken for very short intervals of time.

My observations upon the north coast of Australia are but little satisfactory, both because the changes in the barometer were very small in so low a latitude, and that very little more than the shores of the gulph of Carpentaria could be examined,

on account of the decayed state of the Investigator, which obliged me to return with all practicable expedition to Port Jackson. An abridged statement, however, of the general height of the mercury under the five following circumstances, will afford some light upon the subject, and perhaps not be uninteresting. 1st. On the east side of the gulph, and at the head, with the south-east monsoon, or trade wind. 2d. At the head of the gulph with the north-west monsoon. 3d. On the west side during the north-west monsoon. 4th. At Cape Arnhem under the same circumstance; and 5th. In the passage from Cape Arnhem, at a distance from the coast, to Timor, with variable winds.

In a memoir written by ALEXANDER DALRYMPLE, Esq. F. R. S. respecting the Investigator's voyage, there is this general remark:—" Within the tropics, the monsoon blowing " on the coast produces rainy weather, and when blowing " from over the land, it produces land and sea breezes." This I found verified on the east side of the gulph of Carpentaria, between Nov. 3 and 16, which time was employed in its examination; for though we had found the south-east trade to blow constantly on the east side of Cape York just before, and doubtless it did so then, yet in the gulph we had a tolerably regular sea breeze, which set in from the westward at eleven or twelve o'clock, and continued till seven, eight, or nine in the evening. Towards the head of the gulph, the trade wind, which blew at night and in the morning, came more from the NE, and the sea breezes more from north and NW, but without producing any regular alteration in the height of the mercury, whose average standard was 29,95: it never fell below 29,90 or rose above 30,04. At the head,

the height of the mercury remained nearly the same, until the north-west monsoon began to blow steadily, about the 10th of December, two or three days excepted, when the day winds were from the south-eastward, and the mercury then stood between 29,80 and 29,85. At these times, however, there was usually some thunder and lightning about, signs of the approaching rainy monsoon, which may perhaps account for the descent of the mercury independently of the direction of the wind.

2d. On the confirmation of the north-west monsoon, there was a change in the barometer at the head of the gulph, the common standard of the mercury being at 29,88; but during the times of heavy rain, with thunder, lightning, and squalls of wind, when amongst the islands of Cape Vanderlin, the mean height was 29,79. The north-west monsoon, after coming over Arnhem's land, blows along the shore for a considerable part of the space between the Cape Maria and Cape Van Diemen, of Tasman; and during the examination of the parts so circumstanced, we sometimes had tolerably fine weather, and the mercury above 29,90; but the wind was then usually more from the north than when the mercury stood lower. As we approached Cape Maria, and the bight between it and the south side of Groote Eyland, the mercury stood gradually lower; and in the bight, where the north-west monsoon came directly from off the shore, although we had sea and land breezes, with fine weather, according to Mr. DALRYMPLE's general position, yet the mercury was uncommonly low, its range being from 29,63 to 29,81: the average 29,74, below what it had stood in the very bad weather near Cape Vanderlin. These winds and weather, and

the low state of the mercury, continued until we got without side of Groote Eyland.

3d. On the east side of Groote Eyland, and the west side of the gulph, northward from that island, we sometimes had sea and land breezes with fine weather; we had also two moderate gales of wind from the eastward, of from two to four days continuance each, with one of which there were heavy squalls of wind and rain; sometimes also, the winds were tolerably steady between north and west, with fine weather. During all these variations, the mercury never differed much from its average standard 29,90; and it seemed as if the increase of density in the air, from the wind blowing upon the coast, was equal to its diminution of quantity from the fall of rain and strength of the wind; and on the other side, that the wind from over that corner of Arnhem's Land permitted the mercury to descend, as much as the fine weather would otherwise have occasioned it to rise.

Upon the north side of Groote Eyland, the mercury stood higher than usual for five days, and during this time the wind blew with more regularity from NW, the only exception being for a few hours in the afternoons, when it commonly sprung up from the NE in the manner of a sea breeze: the weather remained fine during these five days, and the height of the mercury averaged 29,94.

4th. In the neighbourhood of Cape Arnhem, the mercury usually stood about 29,90, whether the wind was from NW, NE, or east, if the weather was fine; but if by chance the wind shifted to the south side of west, off the land, it descended to 29,80 though the weather remained the same: and this was its standard during those times when strong gusts

came from the NW accompanied with heavy rain, thunder, and lightning.

In this example, the wind from SW occasioned the mercury to stand lower than that from NW in the same weather; which is contrary to what was observed upon the south and east coasts; particularly on the former, where the south-west wind elevated the mercury up to, and sometimes above, 30,25.

5th. On March 6, 1803, we made sail off from the north coast, towards Timor, the north-west monsoon having ceased to blow at Cape Arnhem, and the eastwardly winds appearing to have set in; but we soon outran them, and had the wind so variable and light afterwards, that it took us twenty-three days to reach Coepang Bay, a distance of no more than 12° of longitude. The only two remarks I made upon the barometer during this passage were, that the common height of the mercury was 29,95 at those times that the wind remained steady for some hours, from whatever quarter it came, and about 29,85 when it was most unsettled; and that it stood higher, upon the average, after we had passed Cape Van Diemen, when the south-west winds, which blew oftenest, came from the sea, than it did before.

The medium height of the mercury, deducting the time between Cape Maria and Groote Eyland in the 2d example, I should take at 29,92, which, when the quantity of rainy squally weather, with thunder and lightning, is considered, is very high: the whole range of the mercury upon the north coast was four-tenths of an inch.

The principal differences in the effect of winds upon this coast, from what they produced upon the south and east coasts, are, that a north-east wind raised the mercury as

high, if not higher, than one from the SE; and that a north-west wind, where it came from off the sea and was moderate, was equal to either of them, and kept it up higher than the south-west wind did.

In order to have ascertained the full effects of sea and land winds upon the barometer, it was desirable to have learned, whether the south-east winds, which occasioned the mercury to rise highest upon the south and east coasts, would have left it at the medium standard, or made it descend upon the north-west and west coasts of Australia; but, unfortunately, the state of the ship did not permit me to determine this; for at the distance we kept from these coasts, in making the best of our way to Port Jackson, the accumulation of air over the shore, arising from a sea wind, or the contrary from a land wind, can scarcely be supposed to have much, if any effect. The principal winds we experienced between Timor and Cape Leuwen, in the months of April and May, were from SE and SW. The south-east wind prevailed as far as the latitude 25°, and the mercury stood at first with it at 29,95; but as we advanced southward, it rose gradually to 30,25, nearly in the same way as it had before descended on the east side of Australia, when we steered northward in the month of October. This wind was succeeded by an unsteady northwardly wind, which brought the mercury down to 29,90; but on its veering by the west to SW it rose fast, and fixed itself about 30,32: we were then drawing near Cape Leuwen.

As far as this example can be admitted in proof, it appears, that a wind from the SW has an equal, if not a superior power to one at SE in raising the mercury upon the west coast; which was not the case upon the south, and still much less

upon the east and north coasts, where the south-west wind caused it to fall. Winds from the northward caused the mercury to descend, as I believe they always will in the southern hemisphere, if not obstructed by the land; but upon the north coast, we have seen the mercury stand higher with it than almost any other.

Upon a summary of the effects of the same winds upon the different coasts of Australia, as deduced from the above examples, the following queries seem to present themselves.

Why do the winds from north and NW, which cause the mercury to descend and stand lower than any other upon the south and east coasts, as also in the open sea, and in the south-west bight of the gulph of Carpentaria, make it rise upon the outer part of the north coast, with the same, or even worse weather?

Why should the north-east wind, which occasions a fall in the barometer upon the south coast, considerably below the mean standard, be attended with a rise above the mean upon the east and north coasts?

The south-east wind, upon the south and east coasts, caused the mercury to rise higher than any other; why should it not have the same effect upon the north coast, and upon the west?

How is it that the south-west wind should make the quicksilver rise and stand high upon the south and west coasts,—should cause it to fall much below the mean standard upon the east coast,—and upon the north, make it descend lower than any other, with the same weather?

The answer, I think, can only be one; and it seems to be sufficiently obvious.

The cause of the sensibility of the mercury to winds blowing from the sea and from off the land, may perhaps admit of more than one explanation; but the following seems to me to be direct, and tolerably satisfactory. The lower air, when brought in by a wind from the sea, meets with resistance in passing over the land; and to overcome this resistance, it is obliged to rise, and will make itself room by forcing the superincumbent air upwards. The first body of air, that thus comes in from the sea, being itself obstructed in its velocity, will obstruct the second, which will therefore rise over the first in like manner, to overcome the obstruction; and as the course of the second body of air will be more direct towards the top of the highest part of the land it has to surmount, than the first was, so the first part of the second body will arrive at the top, before the latter part of the first body has reached it; and this latter part will not be able to pass over the top, being kept down by the second body and the successive stream of air, whose velocity is superior to it. In this manner, an eddy, or body of compressed, and comparatively inactive air will be formed, which, at first, will occupy all the space below a line drawn from the shore to the top of the highest land; but, almost immediately, the succeeding bodies of air, at a distance from the shore, will feel the effect of the obstruction; and being impelled by those that follow them, will begin to rise, taking their course for the top of the highest land, before they come to the shore; by which means, the stratum of lower air will be deeper between the top of the land and the shore, and to some distance out from it, than it is either upon the mountains or in the open sea. If this is admitted to be a necessary consequence of a wind blowing

upon the shore from the sea, it follows, that the mercury ought to stand something higher when such a wind blows, whether it is from the south or any other quarter, than it will with the same wind where it meets no such obstruction; and the more direct it blows upon the coast, and the higher the land is, (all other circumstances being equal,) the higher ought the mercury to rise. On the other hand, when the wind comes from off the hills, this dead and dense air will be displaced, even from its hollows under the highest land; both on account of its own expansion, and because its particles will be attracted by those of the air immediately above, which are taking their unobstructed course out to sea; and thus the air over the coast will resume its natural state with a land wind.

In order to appreciate duly the effect of sea and land winds upon the barometer, in the preceding examples, it is necessary to be recollected, that in the southern hemisphere, a wind from the south has a natural tendency to raise the mercury in the open sea, and one from the north to depress it; probably, from the superior density of the air brought in by the former; therefore, if the mercury rises quicker and higher with a south wind upon the south coast, than it does with a north wind upon the north, it is not to be at once concluded, that the effect of the wind as coming from the sea, is less upon the north coast; for it has, in the first place, to counteract the tendency of the mercury to fall with a north wind; and in some cases, its effects as a sea wind may be as considerable, relatively to the latitude, where there shall be no rise in the barometer, as upon the south coast it might where a considerable one took place. The same thing may be said

of the winds from the east and from the west; for where the vicinity of land is out of the question, the former generally causes an ascent, (from what principle I leave others to determine,) and the latter a descent in the barometer, and I believe this extends to both hemispheres, and all climates. The wind from SE then, which combines something more than half the power, both of the south and of the east wind, will raise the mercury higher than any other, on the south side of the equator, and the wind from NW permit it to fall lower, independently of their effects as sea and land winds; and this allowance requires to be first made upon them: the south-west and north-east quarters should be equal where there is no land in question, and of a medium strength between the power of the south-east, and the deficiency of the north-west wind.

I leave it wholly undetermined, whether the effects of sea and land winds upon the barometer, as above described, extend beyond the shores of the country where these observations were made, and to about one hundred leagues of distance from them; but it seems not improbable, that they may be found to take place near the shores of all countries similarly circumstanced; that is, upon those which are wholly, or for the most part, surrounded by the sea, and situated within the fortieth degree of latitude. In colder climates, where snow lies upon the ground during a part of the year, the wind from off the land may perhaps be so cold, and the air so much condensed, as to produce a contrary effect; but this, and the prosecution of the subject to other important consequences, I leave to the philosopher; my aim being only to supply my

small contribution of raw materials to the hands of the manufacturer, happy if he can make them subservient to the promotion of meteorological science.

I will conclude with stating a few general remarks upon the barometer, such as may be useful to seamen.

It is not so much the absolute, as the relative height of the mercury, and its state of rising and falling, that is to be attended to in forming a judgment of the weather that will succeed; for it appears to stand at different heights, with the same wind and weather, in different latitudes.

In the open sea, it seems to be the changes in the weather, and in the strength of the wind, that principally affect the barometer; but near the shore, a change in the direction of the wind seems to affect it full as much, or more, than either of those causes taken singly.

It is upon the south and east coasts of any country in the southern, or the north and east coasts in the northern hemisphere, where the effect of sea and land winds upon the barometer is likely to be the most conspicuous.

In the open sea, the mercury seems to stand higher in a steady breeze of several days continuance, from whatever quarter it comes, provided it does not blow hard, than when the wind is variable from one part of the compass to another; and perhaps it is on this account, as well as from the direction of the wind, that the mercury stands higher within the tropics, than, upon the average, it appears to do in those parallels where the winds are variable and occasionally blow with violence.

The barometer seems capable of affording so much assis-

tance to the commander of a ship, in warning him of the approach and termination of bad weather, and of changes in the direction of the wind, even in the present state of meteorological knowledge, that no officer in a long voyage should be without one. Some experience is required to understand its language, and it will always be necessary to compare the state of the mercury with the appearance of the weather, before its prognostications will commonly be understood; for a rise may foretel an abatement of wind,—a change in its direction,—or the return of fine weather; or if the wind is light and variable, it may foretel its increase to a steady breeze, especially if there is any easting in it; and a fall may prognosticate a strong breeze or gale, a change of wind, the approach of rain, or the dying away of a steady breeze. Most seamen are tolerably good judges of the appearance of the weather; and this judgment, assisted by observation upon the quick or slower rising or falling of the mercury, and upon its relative height, will in most cases enable them to fix upon which of these changes are about to take place, and to what extent, where there is only one; but a combination of changes will be found more difficult, especially where the effect of one upon the barometer is counteracted by the other; as for instance, the alteration of a moderate breeze from the westward with dull, or rainy weather, to a fresh breeze from the eastward with fine weather, may not cause any alteration in the height of the mercury; though I think there would usually be some rise in this case. Many combinations of changes might be mentioned, in which no alteration in the barometer would be expected, as a little consideration, or

experience in the use of this instrument, will make sufficiently evident; the barometer alone, therefore, is not sufficient; but in assisting the judgment of the seaman, is capable of rendering very important services to navigation.

XI. *Account of a Discovery of native Minium. In a Letter from* James Smithson, *Esq. F. R. S. to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read April 24, 1806.

MY DEAR SIR,

I BEG leave to acquaint you with a discovery which I have lately made, as it adds a new, and perhaps it may be thought an interesting, species to the ores of lead. I have found *minium* native in the earth.

It is disseminated in small quantity, in the substance of a compact carbonate of zinc.

Its appearance in general is that of a matter in a pulverulent state, but in places it shows to a lens a flaky and crystalline texture.

Its colour is like that of factitious minium, a vivid red with a cast of yellow.

Gently heated at the blowpipe it assumes a darker colour, but on cooling it returns to its original red. At a stronger heat it melts to litharge. On the charcoal it reduces to lead.

In dilute white acid of nitre, it becomes of a coffee colour. On the addition of a little sugar, this brown calx dissolves, and produces a colourless solution.

By putting it into marine acid with a little leaf gold, the gold is soon intirely dissolved.

When it is inclosed in a small bottle with marine acid, and

a little bit of paper tinged by turnsol is fixed to the cork, the paper in a short time entirely loses its blue colour, and becomes white. A strip of common blue paper, whose colouring matter is indigo, placed in the same situation undergoes the same change.

The very small quantity which I possess of this ore, and the manner in which it is scattered amongst another substance, and blended with it, have not allowed of more qualities being determined, but I apprehend these to be sufficient to establish its nature.

This native minium seems to be produced by the decay of a galena, which I suspect to be itself a secondary production from the metallization of white carbonate of lead by hepatic gas. This is particularly evident in a specimen of this ore which I mean to send to Mr. GREVILLE, as soon as I can find an opportunity. In one part of it there is a cluster of large crystals. Having broken one of these, it proved to be converted into minium to a considerable thickness, while its centre is still galena.

I am, &c.

JAMES SMITHSON.

Cassell in Hesse,
March 2d, 1806.

XII. *Description of a rare Species of Worm Shells, discovered at an Island lying off the North-west Coast of the Island of Sumatra, in the East Indies. By* J. Griffiths, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *K. B. P.R.S.*

Read February 13, 1806.

A SHORT time after a very violent earthquake that occurred in the island of Sumatra, in the year 1797, these uncommon productions of nature were discovered; the violence of the concussion was more particularly confined to that part of the island situated on the sea coast, between two degrees of the equator north and south, and to the islands adjacent. Its effects were most severely felt at Padang; many lives were lost, and considerable damage sustained, by a most tremendous inundation of the sea; this was also experienced at the low island of Battoo, distant from the coast of Sumatra about twenty leagues.

These shells were procured in a small sheltered bay, with a muddy bottom, surrounded by coral reefs, on the island of Battoo; upon the sea receding from the bay after the inundation they were seen protruding from a bank of slightly-indurated mud, and two or three broken specimens were brought to me at Padang, by the master of a boat trading between that port and the island, for cocoa-nut oil, sea slug, &c.

As I had not observed any of these shells in the cabinets I

had seen abroad, or in England, nor yet a description in any author that I was able to consult, joined to the total ignorance of the Dutch inhabitants of Padang, many of whom had been a long time in the habit of trading to Battoo without having seen or heard of such a production, led me to believe them entirely new, and made me extremely desirous to procure some more perfect specimens, and such information respecting them, as might be acceptable to you, Sir, in your pursuit and inquiry on every subject connected with natural history.

I was consequently induced to send a small praw, with a servant of mine (a Papooa Coffree) who was very expert in diving, and had been employed under my own inspection in procuring many submarine objects, which the coast and islands near Padang abound with; it is therefore from his account, corroborated by others of the crew, that I can give a description of the locality of these subjects, with their appearance in the water, which I think is correct.

He stated, that he had found these tube shells in the bay before mentioned, and in another inlet of the sea, sticking out of rather hard mud, mixed with small stones, sand, &c. from eight to ten inches or more, and from one to three fathoms under water; they were standing in different directions, and separate from each other. Both the master of the boat and crew assured me, that the animal throws out tentacula from the two apertures of the apex of the shell, that resembled the small actiniæ adhering to the rocks about Padang, and that the body of the shell was filled with a soft gelatinous flesh, similar to that of the teredo navalis, but this they had washed out, from its very soon proving putrid, and extremely offensive; that they were in considerable number, and being gently

shaken, easily taken up; but all of them mutilated more or less, which was probably occasioned at the time of the earthquake, when many large fragments of madrepores, corals, &c. were torn from their situation, by the agitation of the sea.

Although more than twenty specimens were brought to me, and others obtained afterwards, there was not one complete; yet being so fortunate as to procure a portion of the shell with the apex nearly perfect, and another with the opposite closed extremity equally so, I am enabled to give a description of them.

The length of the longest of these shells that came into my possession was 5 feet 4 inches, and the circumference at the base 9 inches, tapering upwards to $2\frac{1}{2}$ inches; the colour on the outside milk white, the inner surface rather of a yellow tinge. This specimen was nearly perfect, having a small part of the lower extremity entire. I have others of various dimensions, a very good one about 3 feet long and 4 inches round, tapering to $1\frac{1}{2}$ inch at the point; most of these shells had adhering to them, about one foot or more from the top, the small cockscomb oyster, small serpulæ, &c. consequently they must have been that distance protruded from the hard mud, but the water being thick and discoloured, the people of Battoo had not taken notice of them antecedent to the earthquake.

These tube shells differ very much among themselves, not one of them being correspondent in size or thickness to another. The large end of the shell is completely closed, and has a rounded appearance; at this part it is very thin. The small end or apex is very brittle, and is divided by a longitudinal

septum running down for eight or nine inches, forming it into two distinct tubes, inclosed within the outer one, from whence the animal throws out tentacula; the substance of the shell is composed of layers having a fibrous and radiated appearance, covered externally with a pure white crust, and internally is of a yellow tinge; the external surface is frequently interrupted in a transverse direction by a sudden increase of thickness, which probably indicates different stages in the growth of the shell, although they are at unequal distances, sometimes at six inches, at others four, in the same shell. These interruptions bear a rude and unfinished appearance, and do not extend into the radiated substance, but are merely on the outside shell, which has rather a smooth surface, but at the same time impressed with the irregularities of the substance with which it was in contact. These shells all differ in thickness, some being not more than one-eighth of an inch, others full half an inch in substance; many are nearly straight, others crooked and contorted. The internal surface is in general smooth, though in some of them covered with excrescences resembling tubercles, and without any indication of the animal having adhered to any part of it.

It is the great length and size of these shells, which are the largest of the testacea of a tubular form yet discovered, and the division in the upper part, which constitute their principal peculiarities. I should add, that on their being broken in a transverse direction, the body of the shell between the inner surface and the outer crust, appears to resemble stalactites, and indeed they might easily be mistaken for them.

By consulting RUMPHIUS I found that my opinion of these tubes being entirely a new genus, was unfounded, for which,

Sir, I am much indebted to your kind attention. He is th only original author, I believe, who has given any account of this production, but the figure of the shell in Rumphius is somewhat different from those I have described; it exhibits two long jointed tubes, issuing from the upper part of the exterior tube; and he describes them to be found in shallow water among the mangrove trees; in the account given by Rumphius there is a description both of the ground in which they are found, and the mode in which the large end of the shell is closed, exactly similar to what I have stated, so that it is evidently of the same genus, but, as it differs in having the two tubes through which the tentacula pass out, of considerable length, and entirely separate, a circumstance which may be connected with the situation of the animal in shallow water among mangroves, this, I apprehend, must be clearly admitted to be an entirely new species.

Mr. Home, who has interested himself in the natural history of this animal, has taken the trouble to arrange the drawings for the further illustration of the subject, which have been executed under his inspection from the specimens I have brought to this country; and I shall be happy if my materials, or any I can procure from Sumatra, may enable that gentleman to make any further observations on this curious production of nature.

New Burlington Street,
January 23d, 1806.

EXPLANATION OF THE DRAWINGS. (Plate X.)

Fig. 1, Is a representation of the whole shell in the most perfect state in which its parts have been seen, and there is reason to believe that the only part wanting is the orifice of the double tube. The drawing is made upon a scale of $2\frac{1}{2}$ inches to a foot.

Fig 2, A drawing of the small termination of the shell. At its lower part, for an inch in length, it exhibits the usual appearance of the external surface, but from thence to the end, it is very irregular, and in some specimens small shells of oysters, small serpulæ, &c. adhered to it. All this surface was probably above the mud, exposed to the sea water. At the upper extremity one of the tubes is broken, shewing the size of its canal, also that it is connected with the outer tube in which it is inclosed. The other tube is a little bent, and diverging outwardly, and this is probably its natural termination.

Fig. 3, Represents a section of the shell, at that part where it forms a double tube, to shew the origin of the two tubes, the thickness of the septum between them, and the two orifices leading into them.

Fig. 4, Is a transverse section of the shell at the thickest part, after it had been polished, to shew that it is made up of strata of crystals surrounding one another in concentric circles; also a front view of the orifices into the double tube.

Fig. 5. A front view of the orifices into the double tube, also shewing the thickness of the shell at that part, the canal of which has an oval form.

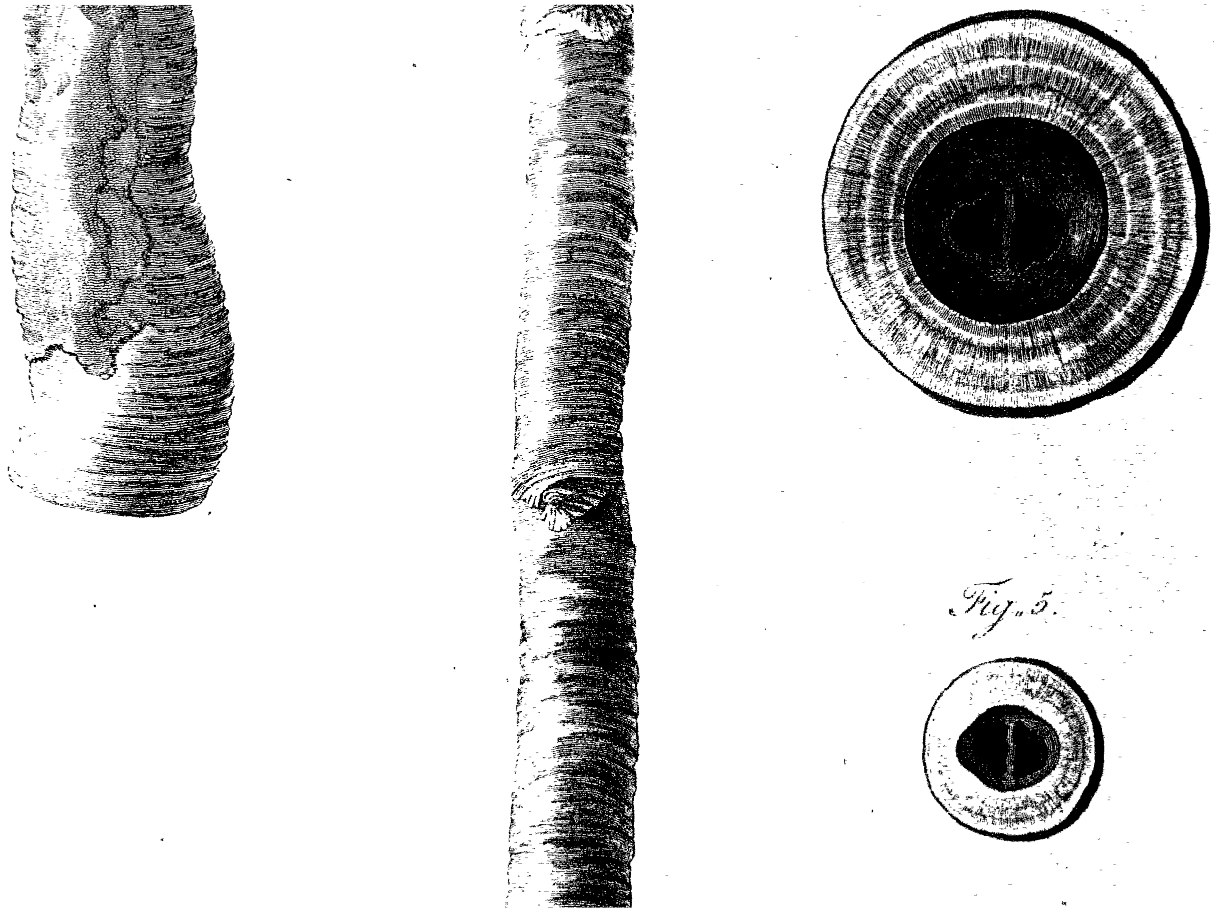
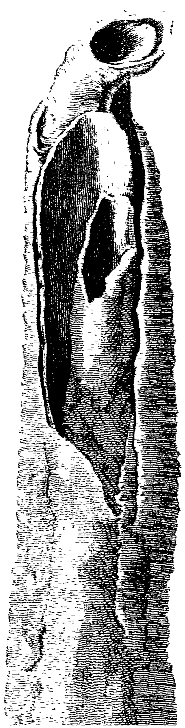
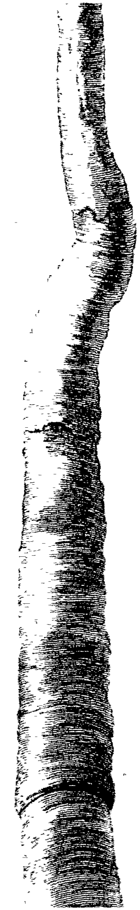
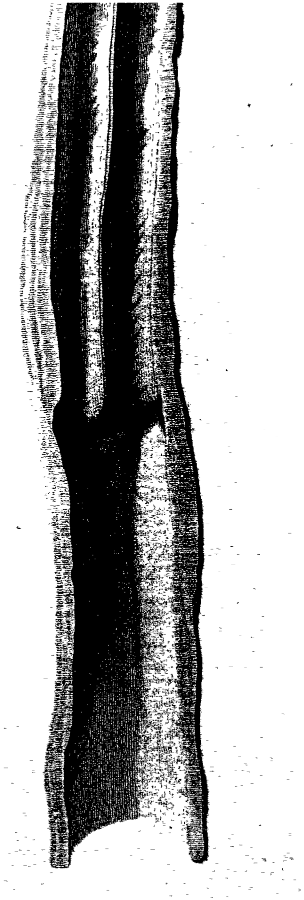


Fig. 5.

Fig. 2.







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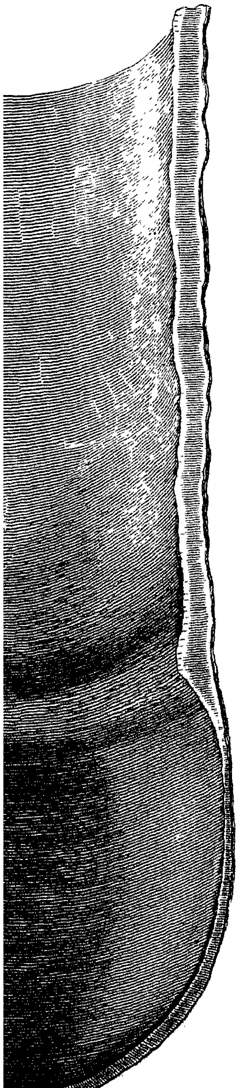


Fig. 7.

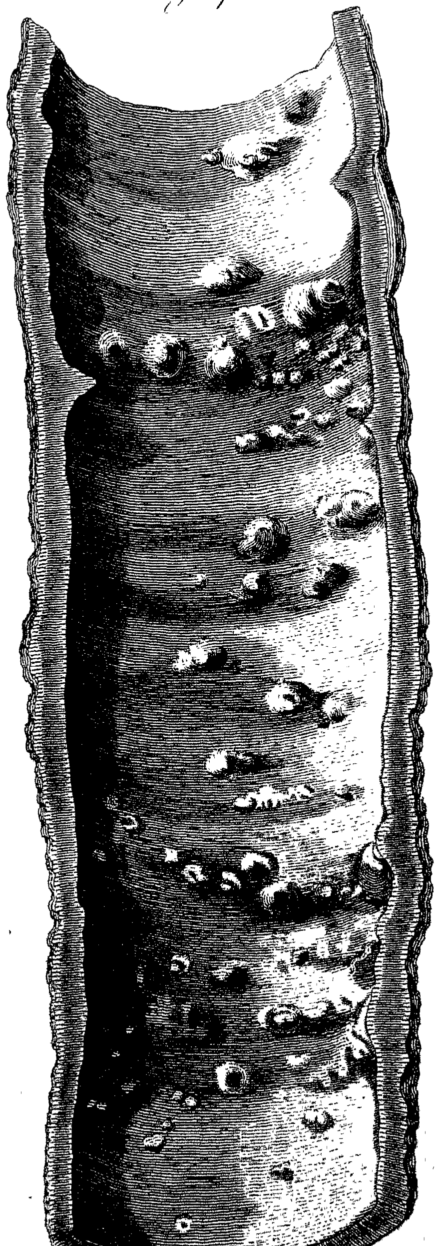


Plate XI.

Fig. 6, Represents the internal cavity of the shell at its lower part, which is every where smooth; it also shews how very thin the shell is which closes up the extremity, compared with that of the general tube, which is also thinnest at the lowest part.

Fig. 7, A section of a specimen which had the tuberculated appearance on its internal surface.

XIII. *Observations on the Shell of the Sea Worm found on the Coast of Sumatra, proving it to belong to a Species of Teredo; with an Account of the Anatomy of the Teredo Navalis. By* Everard Home, *Esq. F. R. S.*

Read May 1, 1806.

THE shell of the sea worm from Sumatra, had only to be seen by any one engaged in comparative anatomy, to arrest his attention, and excite a desire for further information respecting it.

In the spring 1805, Captain MAXWELL of the Calcutta East Indiaman, obligingly gave me a specimen of this shell, five feet long, but imperfect at both extremities. He said it was brought from Sumatra, but could give me no further account of it. The appearance externally, and its radiated structure, led many of my friends to consider it as a mineral substance, formed into a hollow stalactite. Sir JOSEPH BANKS, however, decided on its being the shell of a sea worm. The only means of ascertaining this point then in our power was adopted. It was analysed by Mr. HATCHETT, who found that it was composed of carbonate of lime, and an animal gelatinous substance, which is greater in quantity than in the *chama gigas*, but less than in the common oyster.

Having determined that it was a shell, I applied to Mr. MARSDEN, as the person best acquainted with the natural

history of Sumatra, for further particulars respecting it. He introduced me to his friend Mr. GRIFFITHS, who favoured Sir JOSEPH BANKS with the account, which has already been laid before this learned Society, and also put into my possession a variety of specimens of the shell, to assist me in prosecuting the subject.

There were no facts, by which the genus of the worm, to which this shell belongs, could be ascertained. Sir JOSEPH BANKS, however, had no doubt of its being a teredo. This opinion rendered the subject still more interesting, since it does not, like other teredines, live in wood. The truth of Sir JOSEPH BANKS's opinion has been since established by the discovery of the two boring shells, and the two flattened opercula, which form the decided character of teredines; these were found inclosed in one of the specimens.

The internal structure and economy of teredines are so little known, and much of what is said of them by SELLIUS, the most classical author on that subject, is so vague, that it became necessary to acquire an accurate knowledge of the common teredo navalis, before any adequate idea could be formed of this new species, which may be called Teredo Gigantea.

In this investigation the encouragement and assistance of Sir JOSEPH BANKS were not wanting. By the kindness of Mr. WHITBEY, Master Attendant at Woolwich Yard, and a Fellow of this Society, he procured pieces of wood from Sheerness, in which the animals were alive; at his solicitation the Trustees of the British Museum permitted me, with their usual liberality, to examine a specimen of a teredo preserved in spirits of a very large size: and from the HUNTERIAN

collection another store was opened to me of specimens preserved in spirits.

These opportunities, the able assistance of Mr. Clift, who has been indefatigable in making the drawings, and the aid of Mr. Brodie, have enabled me to draw up the following account of the teredo navalis.

The teredines preserved in salt water lived for three days after being brought to town, which gave me an opportunity of making observations upon them. When the surface of the wood was examined in a good light, while only an inch in depth in sea water, the animal was seen to throw out sometimes one, at others two small tubes. When one only was protruded the other almost immediately followed it. One of these was about $\frac{3}{4}$ of an inch long; the other only half that size. When the largest was exposed to its full extent, there was a fringe on the inside of its external orifice, of about twenty small tentacula, scarcely visible to the naked eye: these were never seen except in that state; for when the tube was retracted, the end was first drawn in, and so on, until the whole was completely inverted: and therefore in a half protruded state it appeared to have a blunt termination, with a rounded edge. The smaller tube was not inverted when drawn in.

These tubes, while playing about in the water appeared at different times to vary in their directions, but usually remained at the greatest convenient distance from each other. The largest was always the most erect, and its orifice the most dilated: the smaller one was sometimes bent on itself with its point touching the wood.

In one instance where a small insect came across the

larger one, the point of the smaller turned round, and pushed it off, and then went back to its original situation.

In several instances the smaller one appeared to be the most sensible: since by touching the larger one gently, it did not retract; but on touching the smaller one they both were instantly drawn in. Indeed whenever they were retracted, they always were drawn in together.

When the worm was confined within the shell, the orifice was not to be distinguished in the irregular surface of the wood, which was covered by small fuci.

The worm appears commonly to bore in the direction of the grain of the wood, but occasionally it bores across the grain, to avoid the track of any of the others: and in some instances there was only a semi-transparent membrane, as a partition between two of them.

In examining the shell while in the wood, its external orifice is very small, just large enough to give a passage to the two small tubes. The sides of the cylinder are thickest near its origin, becoming thinner towards the head of the animal. The greatest thickness met with was $\frac{1}{24}$ of an inch. The canal in the wood at its termination, and for one inch in length, is not lined with shell, but smeared over with a dirty green-coloured mucus, which is also spread upon the last formed portion of shell. The shell was found, when analysed by Mr. HATCHETT, to be perfectly similar to that of the teredo gigantea, being devoid of phosphate of lime, and composed of 97 parts of carbonate of lime, and 3 of animal matter.

While the animal is in the shell, alive and undisturbed, the head is in contact with the end of the canal in the wood; but on laying the head bare, it is drawn in for an inch into the

shell. The body of the animal fills the area of the shell completely: but appears much smaller when taken out, in consequence of the sea water, which it contained, having escaped.

The worms that were examined were of very different lengths. The largest is represented in the annexed drawing, (Plate XII. Fig. 1,) and was 8 inches long. Many of them were alive 24 hours after being removed from their shell: and in these the heart was distinctly seen to palpitate. The blood contained in the vessels going to the head was of a red colour, as also the parts near the liver; but this colour disappeared soon after death.

The head of the worm is inclosed between the two boring shells, which are concave, so that the face, if the expression is admissible, is the only part exposed. The shells in their external form are sufficiently displayed in the drawings, to make a particular description of them unnecessary.

The shells are united together, on what may be called the back part of the head by a very strong digastric muscle, having a middle tendon, from which the fibres go off in a somewhat radiated direction, partly to be inserted into the concave surface of each shell, and partly into a long semicircular process, projecting from the posterior part of each shell. The two inclose the œsaphagus, and other parts surrounding it. The form of the process is shewn in the annexed drawing. The double muscle is inclosed in a smooth shining fascia. When first exposed it was of a bright red colour.

On the opposite side of the head the shells are united by a ligament, from which they are readily separated; at this part there are two small tooth-like processes; one from the narrow edge of each shell, where they are joined together.

From the middle of the exposed part of the head, projects a kind of proboscis: which in the living animal has a vermicular motion: its extremity is covered by a cuticle of a convex form, not unlike the cornea of the eye. When this is removed, the cavity immediately under it is found to contain a hard brown-coloured gelatinous substance, of the form of a Florence flask, with the large end upwards. As this proboscis has no orifice in it, there is reason to believe that it adheres to the wood, acting as a centre bit, while the animal is at work with the shells; and by this means the canal in the wood is so perfectly cylindrical.

The mouth of the animal is nearly concealed by the projection of the proboscis, but when exposed is a very distinct round orifice; between the proboscis and the large digastric muscle.

The body of the worm is inclosed in one general covering, extending from the base of the boring shells, with which it is firmly connected, to the root of the two small tubes, which appear out of the wood. It terminates in a small double fold, forming a cup, on the inside of which are fixed the long small stems of two opercula, which become broad and flat towards their other extremity. These, when brought together, shut up the shell, and inclose the two contracted tubes within it: not one operculum corresponding to each tube, but in a transverse direction. In the teredo gigantea, the opercula are similarly situated, each shutting up one half of the bifurcation.

At the base of this cup the general covering is thick, and ligamentous, for about $\frac{1}{4}$ of an inch in length, where the stems of the opercula are connected with it; and at one spot

of this thickened part, there is an adhesion to the cylindrical shell, which is the only part of the animal connected with it. There is a depression in the shell pointing out this spot. The double fold of the outward covering, that forms the cup, contains the sphincter muscle, which closes the orifice by bringing the opercula together.

The general covering is composed of two membranes, the outer the strongest, and made up of circular fibres, the inner much finer, having no fibrous structure. On the back of the animal, this covering is firmly connected to the parts underneath, and is there strongest. On the belly it forms a cavity, and is thinner. It is every where sufficiently transparent, to shew the different viscera through it.

In examining the internal structure of this worm, the dissection was begun by dividing this covering, and exposing its cavity; into which there are two natural openings: one, that of the largest of the tubes above described, by which it receives water from the sea: the other a transverse slit under the union of the boring shells, $\frac{1}{4}$ of an inch long, opening into the space before the mouth. The smaller tube has no communication with this cavity, nor is there any between this cavity and that of the belly; the viscera having a proper covering of their own: but the breathing organs, which are attached on the posterior surface of this cavity, have their fringed edge, loose, and exposed to the influence of the salt water; so that the larger tube is constantly applying salt water to them, and conveying it to the animal's mouth, through the aperture for that purpose.

The abdominal viscera with the head occupy about one third of the animal's length: the breathing organs another,

and the space between their termination and the ends of the small tubes the remaining third.

In tracing the intestinal canal from the mouth, the œsophagus is found to be very short, and lies on the left side of the neck. On the right side are two large glands near each other, connected with its coat. The œsophagus gradually swells out and becomes stomach, which to external appearance is a large bag, extending the whole length of the abdomen, and the intestine begins close to the termination of the œsophagus: but when the stomach is laid open, there is a septum dividing it into two distinct bags, except at the lower end, where they communicate. It may therefore be said to be doubled on itself. In those worms, which were examined alive, the stomachs were quite empty, but in some preserved specimens the contents were a yellow coloured pulp; and the quantity in that of the specimen from the British Museum was about 10 grains. This pulp was examined by Mr. Hatchett, who considers it to be undoubtedly, an impalpable vegetable sawdust: since when burnt the smoke had precisely the odour of wood, and it formed a charcoal easily consumed, and was converted into white ashes in every respect like vegetable charcoal. Solution of potash did not act upon it, as it would have done had it been an animal substance.

The intestine is extremely small in size; it dilates into a cavity, containing a hard white globular body, of the size of a large pin's head, and then makes a turn upon itself. At this part the liver is attached to the stomach, and adheres so firmly as to be with difficulty separated. The gut passes forwards, till it reaches the central line of the stomach, just opposite the septum, and continues its course along that viscus,

passing round its lower end, and up again on the opposite side. It is then continued on one side of the œsophagus nearly as high as the mouth, where it is reflected over the middle tendon of the digastric muscle of the boring shells, and runs along the back of the animal, till it terminates in the small tube, through which it empties its contents.

The testicles are two long glandular substances, one on each side of the stomach, of a white colour, and granulated structure. From each of them a duct passes to the ovaria, which lie between the two breathing organs. The ducts run upon their outer edge, and terminate near the base of the small tube, and in this way the eggs are impregnated, before they pass out at that orifice.

In the worms examined in February, from Sheerness, the testicles were small, and no appearance of ovaria could be seen: but in some specimens from the Hunterian Museum the testicles were much fuller: and the ovaria formed two distinct longitudinal ridges; which, when examined in the microscope, contained innumerable small eggs; when the ovaria are empty, there is nothing to be found between the two breathing organs but the small seminal vessels.

Sellius mentions that the teredo navalis has its ovaria full of eggs, in the spring and summer: that they are met with as late as December, but those teredines, which he examined in February had their ovaria flaccid and empty.

The heart is situated on the back of the animal; in the middle between the mouth and the lower end of the stomach. It consists of two auricles, composed of a thin dark-coloured membrane; these open by contracted valvular orifices into two white strong tubes, which unite and form the ventricle.

The ventricle may be said to be continued into an artery, which supplies the viscera and goes up to the muscle of the two boring shells. The heart is very loosely connected to the surrounding parts; its action was very distinctly seen through the external covering, and was in some instances continued after it was laid bare. The first contraction is in the two auricles, which shorten themselves in that action. This produces a swelling of the ventricle, followed by a contraction. The artery from the ventricle can be traced up to the head, and the vessels from the auricles are seen very distinctly as far as the breathing organs. The auricles are lined by a black pigment, so that their contents cannot be seen through them, and the ventricle is too thick in its coats to be transparent: but the muscle of the boring shells is of a red colour, as well as the liver, and most of the surrounding parts, between the heart and the head.

This structure of the heart admits only of a single circulation, as in other animals which breathe through the medium of water, but the mode of its being performed is different from that in fishes; in the teredines the blood passes directly from the heart to the different parts of the body, and returns through the vessels of the breathing organs to the heart, while in fishes it goes first to the breathing organs, and then to the different parts of the body.

This peculiar circulation becomes a link in the gradation of the modes of exposing the blood to the air in different animals, it appears to be less perfect than in fishes, since the exposure to the air is carried on more slowly, but is more perfect than in caterpillars.

It is common to animals that have the same general economy

whether their blood has red globules or not, and whether they breathe air or through water. In proof of this it was met with by M. CUVIER in the oyster, in the snail tribe, and all the mollusques which creep on their bellies.*

The mode, in which the breathing organs of the teredines are supplied with water, makes it evident that all sea worms, as well as other soft animals, which have no cavity for the reception of sea water, must have the breathing organs placed externally. This is the case with all those species of *Actiniæ* met with in the West Indies, called Animal Flowers; and the beautiful membranous expansions they display resembling the petals of flowers, are in fact the breathing organs, not tentacula for catching food, as their appearance led me to believe, when describing the new species, discovered in the year 1780, and which has a place in the Philosophical Transactions for 1785.

In animals so perfect in their organs as the teredines, and which have red blood, there can be no doubt of the existence of brain and nerves: but it is not to be wondered at that from the gelatinous texture of the animal they eluded every attempt to discover them, in the present investigation.

There was no material difference in the structure of the different varieties that were examined, although they varied from each other exceedingly in their size: except that in the large one from the British Museum, the heart was situated almost close to the origin of the breathing organs. All of them had vegetable matter in their stomachs. They must therefore all be inhabitants of wood, and belong to one species.

* Vide *Leçons d'Anatomie comparée*, Vol. IV. Lec. 27.

The teredo gigantea is imbedded in a different substance, and may have many other characteristic differences: although it appears from comparing the shells in which they are incased, that they are formed of exactly the same materials.

The teredo gigantea, when arrived at its full growth closes up the end of the shell. This the teredo navalis does also; and SELLIUS was induced to believe that the animal, by this act, formed its own tomb, since it could no longer destroy the wood, in which it was contained. We find, however, that in the teredo gigantea death is not a consequence of this seclusion from the substance, in which it is imbedded.

In some of the specimens in Mr. GRIFFITHS's possession, the shell is just covered in, and that part close to the termination is extremely thin, but in others is increased in thickness twenty fold: in others again the shell has not only become thick, but the animal has receded from its first inclosure, and has formed a second three inches up the tube, and afterwards a third two inches further on, and has made the sides thicker and thicker, to diminish the canal in proportion to the diminution of its own size.

These facts prove that the teredo gigantea, when arrived at its full growth, or whenever prevented from increasing its length, closes up the end of its shell, and lives a long time afterwards, furnished with food from the sea-water it receives like the actineæ. The teredo navalis closes up its shell in the same manner; it must, therefore, after that period, be supplied with food entirely through the medium of sea-water.

The teredines in their anatomical structure are more perfect than many of the other vermes, and have a portion of red

blood. They turn round in their shell, with which the body has no attachment, and with which their covering only has a slight connection, at one particular spot, to prevent the external tubes from being disturbed. This motion of the animal is for the purpose of boring.

Their most striking peculiarities are, having three external openings instead of two: the stomach being unusually large, and the breathing organs having an uncommon conformation.

As the teredo gigantea bores in mud, on which it cannot be supposed to subsist, or even to receive any part of its nutriment from it, a question arises whether the teredo navalis (an animal of a much smaller size) receives its support from the wood it destroys, or is wholly supplied with food from the sea.

The following observations make the last opinion by much the most probable. The animal having red blood, and very perfect organs, necessarily requires a great deal of nourishment for the purposes of growth, and to supply the waste constantly going on: but if the aggregate of shell and animal substance is taken, it will be found equal in bulk, and greater in specific gravity, than the wood displaced in making the hole: hence it is obvious that the quantity of wood, it has taken into its body, is wholly insufficient for its formation and subsequent support. It must therefore have other means of subsistence. When once it is established that the worm can be supported, independantly of the wood, which is eaten, and can afterwards subsist, when the communication between it and the wood is cut off, it creates a doubt respecting the wood forming any part of its aliment, and makes it probable that

the teredo navalis, like the teredo gigantea, forms its habitation in a substance from which it receives no part of its sustenance: and that the sawdust conveyed through the intestines is not digested, particularly as that examined by Mr. HATCHETT, had not undergone the slightest change.

The straight course of the intestine in the teredines makes it probable that the sawdust retards the progress of the food, so as to render convolutions unnecessary. In some of the actineæ from the West Indies, the intestine is so much convoluted, that it appears to be wound round a central cylinder, in closely compacted turns.

EXPLANATION OF THE PLATES.

Plate XII.

Fig. 1, Represents a portion of the teredo navalis in its shell inclosed in the wood, to show the manner in which the two tubes are protruded, and the appearance of the shell at its termination, which is contracted but not divided into two canals as in that of the teredo gigantea.

Fig. 2, Represents the teredo belonging to the British Museum, the opercula are wanting, and the tubes are retracted.

aa, Are the boring shells.

b, The proboscis.

c, The mouth.

dd, The contents of the abdomen seen through the transparent external covering.

ee, The breathing organs seen in the same way.

Fig. 3. The teredo navalis from Sheerness, with the tubes protruded and the opercula in their situation. The letters denote the same parts as in Fig. 2. In this figure the cup containing the opercula and tubes is distinctly seen, and these parts are represented in their natural situation.

Fig. 4, Represents the external surface of one of the opercula of the teredo gigantea.

Fig. 5, Shows the other side of the same operculum.

Fig. 6, Shows a side view of the boring shell of the same teredo with the process that projects from its concave surface, and its cutting edge.

Fig 7 and 8, Show the two sides of one of the opercula of the teredo navalis.

Fig. 9 and 10, Show two views of the boring shell of the same teredo.

All these figures are of the natural size of the parts they represent.

Plate XIII.

In this plate are three figures of the teredo from the British Museum, to show its internal structure; the different parts are represented of their natural size.

Fig. 1, Represents the animal laid open through the whole extent, exposing the abdominal view.

aa, The boring shells.

bbbb, The external covering divided and turned back.

c, The larger tube, which conveys the sea-water into that cavity, in its completely retracted state.

d, The orifice, by which the sea-water passes out, between the boring shells and the proboscis into the space before the mouth.

e, The œsophagus.

f, Two glands which lie upon it.

gg, The stomach.

h, The liver.

ii, A portion of one of the testicles.

k, The beginning of the intestine.

ll, The intestine passing down upon the stomach.

mm, The breathing organs.

nn, The two ovaria between them.

oo, The intestine leading to its termination in the small tube behind the large one.

Fig. 2, Represents the course of the stomach and intestines removed from the body.

a, The œsophagus.

b, The stomach.

c, The septum, dividing it into two cavities.

d, The aperture by which the two cavities of the stomach communicate.

eeee, The course of the intestine to its termination.

Fig. 3, The internal structure of the animal exposed in a posterior view.

aa, The two boring shells, separated from each other and turned back.

b, The digastric muscle.

c, The intestine passing over it, and cut off to expose the other parts.

dd, The two testicles.

ee, The auricles of the heart.

ff, The ventricle of the heart.

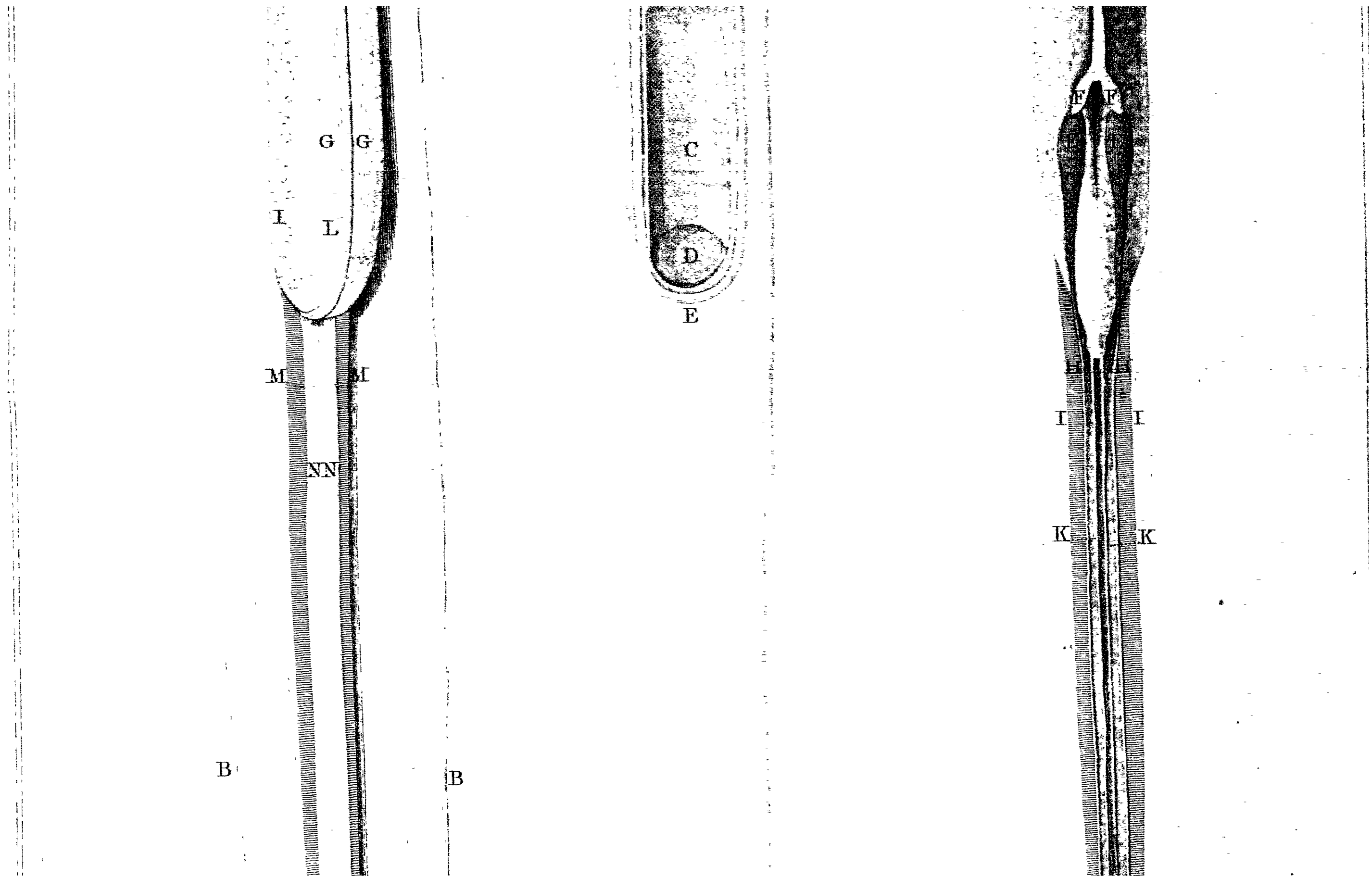
gg, The artery going to the head.

hh, The vessels coming from the breathing organs to the heart.

ii, The breathing organs.

kkkk, The ducts of the testicles.

ll, A strong substance with transverse fibres, having a pile upon it, to strengthen this, which is the weakest part of the animal.



G G
L L
M M
NN
B B
C
D
E
F F
H H
I I
K K

Fig. 1.

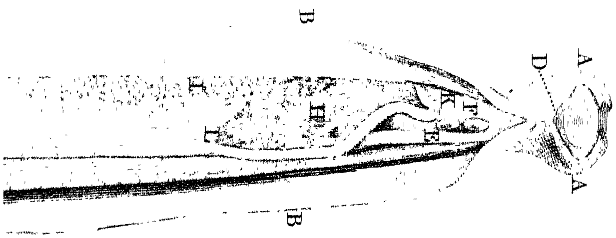


Fig. 2.

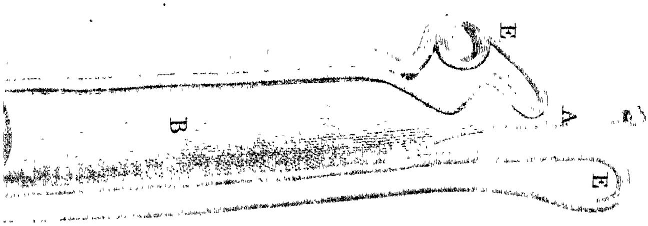
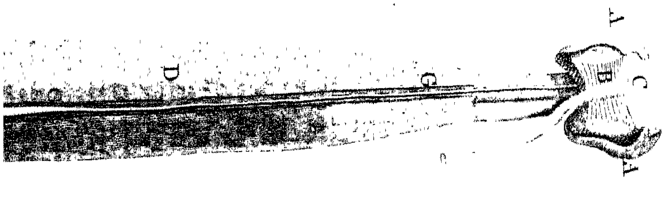


Fig. 3.



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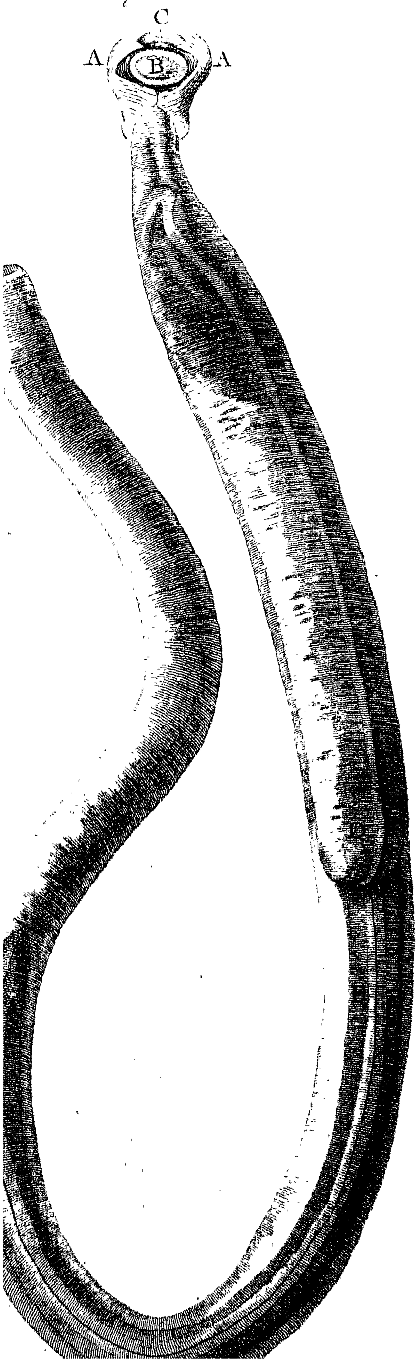

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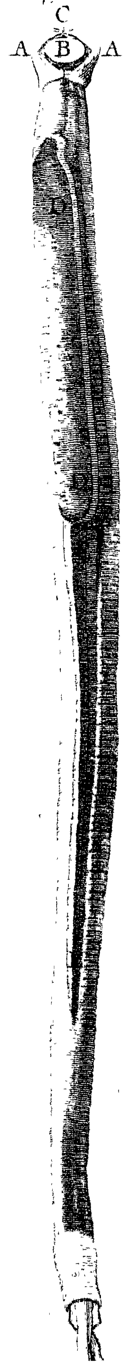

Fig. 4.

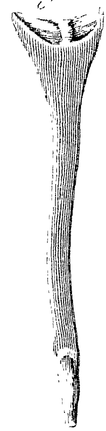

Fig. 5

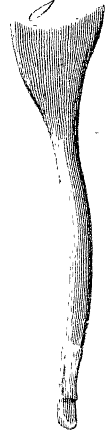

Fig. 6.

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XIV. *On the inverted Action of the alburnous Vessels of Trees. By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read May 15, 1806.

MY DEAR SIR,

I HAVE endeavoured to prove, in several Memoirs* which you have done me the honour to lay before the Royal Society, that the fluid by which the various parts (that are annually added to trees, and herbaceous plants whose organization is similar to that of trees,) are generated, has previously circulated through their leaves† either in the same, or preceding season, and subsequently descended through their bark; and after having repeated every experiment that occurred to me, from which I suspected an unfavourable result, I am not in possession of a single fact which is not perfectly consistent with the theory I have advanced.

There is, however, one circumstance stated by HALES and

* In the Phil. Trans. for 1801, 1803, 1804, and 1805.

† During the circulation of the sap through the leaves, a transparent fluid is emitted, in the night, from pores situated on their edges; and on evaporating this liquid obtained from very luxuriant plants of the vine I found a very large residuum to remain, which was similar in external appearance to carbonate of lime. It must, however, have been evidently a very different substance from the very large portion, which the water held in solution. I do not know that this substance has been analyzed, or noticed by any naturalist

Du Hamel which appears strongly to militate against my hypothesis; and as that circumstance probably induced Hales to deny altogether the existence of circulation in plants, and Du Hamel to speak less decisively in favour of it than he possibly might otherwise have done, I am anxious to reconcile the statements of these great naturalists, (which I acknowledge to be perfectly correct,) with the statements and opinions I have on former occasions communicated to you.

Both Hales and Du Hamel have proved, that when two circular incisions through the bark, round the stem of a tree, are made at a small distance from each other, and when the bark between these incisions is wholly taken away, that portion of the stem which is below the incisions through the bark continues to live, and in some degree to increase in size, though much more slowly than the parts above the incisions. They have also observed that a small elevated ridge (*bourvelet*) is formed round the lower lip of the wound in the bark, which makes some slight advances to meet the bark and wood projected, in much large quantity, from the opposite, or upper lip of the wound.

I have endeavoured, in a former Memoir,* to explain the cause why some portion of growth takes place below incisions through the bark, by supposing that a small part of the true sap, descending from the leaves, escapes downwards through the porous substance of the alburnum. Several facts stated by Hales seem favourable to this supposition; and the existence of a power in the alburnum to carry the sap in different directions, is proved in the growth of inverted cuttings of different species of trees.† But I have derived so

* Phil. Trans. for 1803. † Ibid. for 1804.

many advantages, both as a gardener and farmer, (particucularly in the management of fruit and forest trees,) from the experiments which have been the subject of my former memoirs, that I am confident much public benefit might be derived from an intimate acquaintance with the use and office of the various organs of plants; and thence feel anxious to adduce facts to prove that the conclusions I have drawn are not inconsistent with the facts stated by my great predecessors.

It has been acknowledged, I believe, by every naturalist who has written on the subject, (and the fact is indeed too obvious to be controverted,) that the matter which enters into the composition of the radicles of germinating seeds existed previously in their cotyledons; and as the radicles increase only in length by parts successively added to their apices, or points most distant from their cotyledons, it follows of necessity that the first motion of the true sap, at this period, is downwards. And as no alburnous tubes exist in the radicles of germinating seeds during the earlier periods of their growth, the sap in its descent must either pass through the bark, or the medulla. But the medulla does not apparently contain any vessels calculated to carry the descending sap; whilst the cortical vessels are during this period much distended and full of moisture: and as the medulla certainly does not carry any fluid in stems or branches of more than one year old, it can scarcely be suspected that it, at any period, conveys the whole current of the descending sap.

As the leaves grow, and enter on their office, cortical vessels, in every respect apparently similar to those which descended from the cotyledons, are found to descend from the bases of the leaves; and there appears no reason, with

which I am acquainted, to suspect that both do not carry a similar fluid, and that the course of this fluid is, in the first instance, always towards the roots.

The ascending sap, on the contrary, rises wholly through the alburnum and central vessels; for the destruction of a portion of the bark, in a circle round the tree, does not immediately in the slightest degree check the growth of its leaves and branches: but the alburnous vessels appear, from the experiments I have related in a former Paper,* and from those I shall now proceed to relate, to be also capable of an inverted action, when that becomes necessary to preserve the existence of the plant.

As soon as the leaves of the oak were nearly full grown in the last spring, I selected in several instances two poles of the same age, and springing from the same roots in a coppice, which had been felled about six years preceding; and making two circular incisions at the distance of 3 inches from each other through the bark of one of the poles on each stool, I destroyed the bark between the incisions, and thus cut off the communication between the leaves and the lower parts of the stem and roots, through the bark. Much growth, as usual, took place above the space from which the bark had been taken off, and very little below it.

Examining the state of the experiment in the succeeding winter, I found it had not succeeded according to my hopes; for a portion of the alburnum, in almost every instance, was lifeless, and almost dry, to a considerable distance below the space from which the bark had been removed. In one instance the whole of it was, however, perfectly alive; and in

* Phil. Trans. for 1804.

this I found the specific gravity of the wood above the decorticated space to be 1114, and below it 1111; and the wood of the unmutilated pole at the same distance from the ground to be 1112, each being weighed as soon as it was detached from the root.

Had the true sap in this instance wholly stagnated above the decorticated space, the specific gravity of the wood there ought to have been, according to the result of former experiments,* comparatively much greater; but I do not wish to draw any conclusion from a single experiment; and indeed I see very considerable difficulty in obtaining any very satisfactory, or decisive facts from any experiments on plants, in this case, in which the same roots and stems collect and convey the sap during the spring and summer, and retain, within themselves, that which is, during the autumn and winter, reserved to form new organs of assimilation in the succeeding spring. In the tuberous-rooted plants, the roots and stems which collect and convey the sap in one season, and those in which it is deposited and reserved for the succeeding season, are perfectly distinct organs; and from one of these, the potatoe, I obtained more interesting and decisive results.

My principal object was to prove that a fluid descends from the leaves and stem to form the tuberous roots of this plant; and that this fluid will in part escape down the alburnous substance of the stem when the continuity of the cortical vessels is interrupted: but I had also another object in view.

Every gardener knows that early varieties of the potatoe never afford either blossoms or seeds; and I attributed this

* Phil. Trans. for 1805.

peculiarity to privation of nutriment, owing to the tubers being formed preternaturally early, and thence drawing off that portion of the true sap, which in the ordinary course of nature is employed in the formation and nutrition of blossoms and seeds.

I therefore planted, in the last spring, some cuttings of a very early variety of the potatoe, which had never been known to blossom, in garden pots, having heaped the mould as high as I could above the level of the pot, and planted the portion of the root nearly at the top of it. When the plants had grown a few inches high, they were secured to strong sticks, which had been fixed erect in the pots for that purpose, and the mould was then washed away from the base of their stems by a strong current of water. Each plant was now suspended in air, and had no communication with the soil in the pots except by its fibrous roots, and as these are perfectly distinct organs from the runners which generate and feed the tuberous roots, I could readily prevent the formation of them. Efforts were soon made by every plant to generate runners and tuberous roots; but these were destroyed as soon as they became perceptible. An increased luxuriance of growth now became visible in every plant, numerous blossoms were emitted, and every blossom afforded fruit.

Conceiving, however, that a small part only of the true sap would be expended in the production of blossoms and seeds, I was anxious to discover what use nature would make of that which remained; and I therefore took effectual means to prevent the formation of tubers on any part of the plants, except the extremities of the lateral branches, those being the points most distant from the earth, in which the tubers are

naturally deposited. After an ineffective struggle of a few weeks, the plants became perfectly obedient to my wishes, and formed their tubers precisely in the places I had assigned them. Many of the joints of the plants during the experiment became enlarged and turgid; and I am much inclined to believe, that if I had totally prevented the formation of regular tubers, these joints would have acquired an organization capable of retaining life, and of affording plants in the succeeding spring.

I had another variety of the potatoe, which grew with great luxuriance, and afforded many lateral branches; and just at that period, when I had ascertained the first commencing formation of the tubers beneath the soil, I nearly detached many of these lateral branches from the principal stems, letting them remain suspended by such a portion only of alburnous and cortical fibres and vessels as were sufficient to preserve life. In this position I conceived that if their leaves and stems contained any unemployed true sap, it could not readily find its way to the tuberous roots, its passage being obstructed by the rupture of the vessels, and by gravitation; and I had soon the pleasure to see that instead of returning down the principal stem into the ground, it remained and formed small tubers at the base of the leaves of the depending branches.

The preceding facts are, I think, sufficient to prove that the fluid, from which the tuberous root of the potatoe, when growing beneath the soil, derives its component matter, exists previously either in the stems or leaves; and that it subsequently descends into the earth: and as the cortical vessels during every period of the growth of the tuber are filled with

the true sap of the plant, and as these vessels extend into the runners, which carry nutriment to the tuber, and in other instances evidently convey the true sap downwards, there appears little reason to doubt that through these vessels the tuber is naturally fed.

To ascertain, therefore, whether the tubers would continue to be fed when the passage of the true sap down the cortical vessels was interrupted, I removed a portion of bark of the width of five lines, and extending round the stems of several plants of the potatoe, close to the surface of the ground, soon after that period when the tubers were first formed. The plants continued some time in health, and during that period the tubers continued to grow, deriving their nutriment, as I conclude, from the leaves by an inverted action of the alburnous vessels. The tubers, however, by no means attained their natural size, partly owing to the declining health of the plant, and partly to the stagnation of a portion of the true sap above the decorticated space.

The fluid contained in the leaf has not, however, been proved, in any of the preceding experiments, to pass downwards through the decorticated space, and to be subsequently discharged into the bark below it: but I have proved with amputated branches of different species of trees that the water which their leaves absorb, when immersed in that fluid, will be carried downwards by the alburnum, and conveyed into a portion of bark below the decorticated space; and that the insulated bark will be preserved alive and moist during several days;* and if the moisture absorbed by a leaf can be

* This experiment does not succeed till the leaf has attained its full growth and maturity, and the alburnum of the annual shoot its perfect organization.

thus transferred, it appears extremely probable that the true sap will pass through the same channel. This power in the alburnum to carry fluids in different directions probably answers very important purposes in hot climates, where the dews are abundant and the soil very dry; for the moisture the dews afford may thus be conveyed to the extremities of the roots: and HALES has proved that the leaves absorb most when placed in humid air; and that the sap descends, either through the bark or alburnum, during the night.

If the inverted action of the alburnous vessels in the decorticated space be admitted, it is not difficult to explain the cause why some degree of growth takes place below such decorticated spaces on the stems of trees; and why a small portion of bark and wood is generated on the lower lip of the wound. A considerable portion of the descending true sap certainly stagnates above the wound, and of that which escapes into the bark below it, the greater part is probably carried towards, and into, the roots; where it preserves life, and occasions some degree of growth to take place. But a small portion of that fluid will be carried upwards by capillary attraction, between the bark and the alburnum, exclusive of the immediate action of the latter substance, and the whole of this will stagnate on the lower lip of the wound; where I conceive it generates the small portion of wood and bark, which HALES and DU HAMEL have described.

I should scarely have thought an account of the preceding experiments worth sending to you, but that many of the conclusions I have drawn in former memoirs appear, at first view, almost incompatible with the facts stated by HALES and

DU HAMEL, and that I had one fact to communicate relative to the effects produced by the stagnation of the descending sap of resinous trees, which appeared to lead to important consequences. I have in my possession a piece of a fir-tree, from which a portion of bark, extending round its whole stem, had been taken off several years before the tree was felled; and of this portion of wood one part grew above, and the other below, the decorticated space. Conceiving that, according to the theory I am endeavouring to support, the wood above the decorticated space ought to be much heavier than that below it, owing to the stagnation of the descending sap, I ascertained the specific gravity of both kinds, taking a wedge of each as nearly of the same form, as I could obtain, and I found the difference greatly more than I had anticipated, the specific gravity of the wood above the decorticated space being 0.590, and of that below only 0.491: and having steeped pieces of each, which weighed a hundred grains, during twelve hours in water, I found the latter had absorbed 69 grains, and the former only 51.

The increased solidity of the wood above the decorticated space, in this instance, must, I conceive have arisen from the stagnation of the true sap in its descent from the leaves; and therefore in felling firs, or other resinous trees, considerable advantages may be expected from stripping off a portion of their bark all round their trunks, close to the surface of the ground, about the end of May or beginning of June, in the summer preceding the autumn in which they are to be felled. For much of the resinous matter contained in the roots of these is probably carried up by the ascending sap in the

spring, and the return of a large portion of this matter to the roots would probably be prevented:* the timber I have, however, very little doubt would be much improved by standing a second year, and being then felled in the autumn; but some loss would be sustained owing to the slow growth of the trees in the second summer. The alburnum of other trees might probably be rendered more solid and durable by the same process; but the descending sap of these, being of a more fluid consistence than that of the resinous tribe, would escape through the decorticated space into the roots in much larger quantity.

It may be suspected that the increased solidity of the wood in the fir-tree I have described was confined to the part adjacent to the decorticated space; but it has been long known to gardeners, that taking off a portion of bark round the branch of a fruit-tree occasions the production of much blossom on every part of that branch in the succeeding season. The blossom in this case probably owes its existence to a stagnation of the true sap extending to the extremities of the branch above the decorticated space; and it may therefore be expected that the alburnous matter of the trunk and branches of a resinous tree will be rendered more solid by a similar operation.

* The roots of trees, though of much less diameter than their trunks and branches, probably contain much more alburnum and bark, because they are wholly without heart wood, and extend to a much greater length than the branches; and thence it may be suspected that when fir-trees are felled, their roots contain at least as much resinous matter, in a fluid moveable state, as their trunks and branches; though not so much as is contained, in a concrete state, in the heart wood of those.

I send you two specimens of the fir-wood I have described, the one having been taken off above, and the other below, the decorticated space. The bark of the latter kind scarcely exceeded one-tenth of a line in thickness; the cause of which I propose to endeavour to explain in a future communication relative to the reproduction of bark.

I am, &c.

T. A. KNIGHT.

XV. *A new Demonstration of the Binomial Theorem, when the Exponent is a positive or negative Fraction. By the Rev.* Abram Robertson, *A. M. F. R. S. Savilian Professor of Geometry in the University of Oxford. In a Letter to* Davies Giddy, *Esq. F. R. S.*

Read June 5, 1806.

DEAR SIR,

Being perfectly convinced of your love of mathematical science, and your extensive acquirements in it, I submit to your perusal a new demonstration of the binomial theorem, when the exponent is a positive or negative fraction. As I am a strenuous advocate for smoothing the way to the acquisition of useful knowledge, I deem the following articles of some importance; and unless I were equally sincere in this persuasion, and in that of your desire to promote mathematical studies, in requesting the perusal, I should accuse myself of an attempt to trifle with your valuable time.

The following demonstration is new only to the extent above mentioned; but in order that the reader may perceive the proof to be complete, a successive perusal of all the articles is necessary. As far as it relates to the raising of integral powers, it is in substance the same with one which I drew up in the year 1794, and which was honoured with a place in the Philosophical Transactions for 1795. If, therefore,

you think the following demonstration worthy the attention of mathematicians, you will much oblige me by presenting it to the Royal Society.

I am, &c.

A. ROBERTSON.

Oxford,
March 21st, 1806.

1. The binomial theorem is a general expression for any power of the sum or difference of two quantities. Thus if n be any positive or negative whole number, or vulgar fraction, and a, b, be any two quantities, the binomial theorem expresses in a series the value of $\overline{a+b}|^n$, or $\overline{a-b}|^n$.

The binomial theorem is of very extensive utility. Besides the advantages derived from it in raising powers and extracting roots, it enables us to conduct, with clearness and ease, a variety of investigations in the higher parts of algebra, which, without its assistance, would become perplexed and laborious.

2. If n be a whole positive number, we can raise $x+a$ to the power denoted by n, in the following manner, by multiplication.

$$
\begin{array}{l}
x+a \\
x+a \\
\hline
x^2+ax \\
\quad\; ax+a^2 \\
\hline
x^2+2ax+a^2=\overline{x+a}|^2 \\
x+a \\
\hline
x^3+2ax^2+a^2x \\
\quad\;\; ax^2+2a^2x+a^3 \\
\hline
x^3+3ax^2+3a^2x+a^3=\overline{x+a}|^3 \\
x+a \\
\hline
x^4+3ax^3+3a^2x^2+a^3x \\
\quad\;\; ax^3+3a^2x^2+3a^3x+a^4 \\
\hline
x^4+4ax^3+6a^2x^2+4a^3x+a^4=\overline{x+a}|^4, \\
\qquad\qquad \&c.
\end{array}
$$

In the same manner the value of $\overline{x-a}|^n$ may be obtained; and its only difference from the value of $\overline{x+a}|^n$ will consist in having the negative sign prefixed to such terms as have an odd power of a. And as the powers of any other quantity, either simple or compound, may be obtained gradually by multiplying the last found power by the root, in order to find the next higher power, it is manifest that the principles of multiplication are the most simple and evident, to which we can resort, for the demonstration of the binomial theorem. These principles, therefore, will be used throughout the whole of the following investigations on the subject, and by them every case of the theorem will be established.

It is well known to mathematicians that the theorem has been repeatedly proved, either by induction, by the summation of figurate numbers, by the doctrine of combinations, by assumed series, or by fluxions; but that multiplication is a more direct way to the establishment of the theorem than any of these, cannot, I think, be doubted. Proceeding by multiplication, we have always an evident first principle in view, to which without the aid of any doctrine, foreign to the subject, we can appeal for the truth of our assertions, and the certainty and extent of our conclusions.

3. If p, q, be any two quantities, the product arising from the multiplication of p by q is equal to the product arising from the multiplication of q by p.* For magnitudes being to one another as their equimultiples, $p \times q : 1 \times q :: p : 1$, and $q \times p : 1 \times p :: q : 1$. But $1 \times q = q$, and $1 \times p = p$, and therefore, placing for ex æquali in a cross order,

$$p \times q : q : 1$$
$$q \times p : p : 1.$$

Consequently, $p \times q : 1 :: q \times p : 1$, and therefore $pq = qp$.

Hence it follows that the product arising from the multiplication of any number of quantities into one another, continues the same in value, in every variation which may be made in the arrangement of the quantities which compose it. Thus p, q, r, s, being any quantities, $pqrs = pqr \times s = spqr = spq \times r = rspq = rsp \times q = qrsp = qr \times s \times p = qr \times p \times s = qrps$, &c. And if $x + a = p$, $x + b = q$, $x + c = r$, $x + d = s$, $x + e = t$, &c. then $\overline{x+a} \times \overline{x+b} \times \overline{x+c} \times \overline{x+d} \times \overline{x+e} = pqrst = \overline{x+a} \times \overline{x+b} \times$

* When I speak of the multiplication of quantities into one another, I mean the multiplication of the numbers into one another which measure those quantities.

$\overline{x+c} \times \overline{x+e} \times \overline{x+d} = pqrts =$ any other arrangement which can take place in the quantities.

4. It is evident that each of the quantities $a, b, c,$ &c. will be found the same number of times in the compound product arising from $\overline{x+a} \times \overline{x+b} \times \overline{x+c} \times \overline{x+d} \times \overline{x+e}$, &c. For this product is equal to $pqrst = pqrs \times \overline{x+e} = pqrt \times \overline{x+d} = pqst \times \overline{x+c} = prst \times \overline{x+b} = qrst \times \overline{x+a}$, by substituting for the compound quantities, $x+a$, $x+b$, &c. their equals p, q, &c. Wherefore, in the compound product, each of the quantities $a, b, c,$ &c. will be found multiplied into the products of all the others.

5. These things being premised, we may poceed to the multiplication of the compound quantities $x+a$, $x+b$, $x+c$, &c. into one another; and in order to be as clear as possible in what follows, let us consider the sum of the quantities, a, b, c, &c. or the sum of any number of them multiplied into one another, as coefficients to the several powers of x, which arise in the multiplication. By considering products which contain the same number of the quantities $a, b, c,$ &c. as homologous, the multiplication will appear as follows, and equations of various dimensions will arise, according to the powers of x.

$$\begin{array}{l} x+a=p \\ x+b=q \\ \hline \end{array}$$

$$x^2\left.\begin{array}{l}+a\\+b\end{array}\right\}x+ab=pq;$$ a quadratic equation, or an equation of two dimensions.

$$\begin{array}{l} x+c=r \\ \hline \end{array}$$

$$x^3\left.\begin{array}{l}+a\\+b\\+c\end{array}\right\}x^2\left.\begin{array}{l}+ab\\+ac\\+bc\end{array}\right\}x+abc=pqr;$$ a cubic, or an equation of three dimensions.

$$\begin{array}{l} x+d=s \\ \hline \end{array}$$

$$x^4\left.\begin{array}{l}+a\\+b\\+c\\+d\end{array}\right\}x^3\left.\begin{array}{l}+ab\\+ac\\+bc\\+ad\\+bd\\+cd\end{array}\right\}x^2\left.\begin{array}{l}+abc\\+abd\\+acd\\+bcd\end{array}\right\}x+abcd=pqrs;$$ a biquadratic, or an equation of four dimensions.

$$\begin{array}{l} x+e=t \\ \hline \end{array}$$

$$x^5\left.\begin{array}{l}+a\\+b\\+c\\+d\\+e\end{array}\right\}x^4\left.\begin{array}{l}+ab\\+ac\\+bc\\+ad\\+bd\\+cd\\+ae\\+be\\+ce\\+de\end{array}\right\}x^3\left.\begin{array}{l}+abc\\+abd\\+acd\\+bcd\\+abe\\+ace\\+bce\\+ade\\+bde\\+cde\end{array}\right\}x^2\left.\begin{array}{l}+abcd\\+abce\\+abde\\+acde\\+bcde\end{array}\right\}x+abcde=pqrst;$$ a sur-solid, or an equation of five dimensions.

&c.

6. From the above it appears, that the coefficient of the highest power of x in any equation is 1; but the coefficient of any other power of x in the same equation consists of a certain number of members, each of which contains one, two, three, &c. of the quantities a, b, c, &c. Thus the coefficient of

the second term of any equation is made up of members, each of which contains only one of the quantities a, b, c, &c. and the whole coefficient of the second term is the sum of all these members, or the sum of all the quantities a, b, c, &c. used in the multiplication by which the equation, under consideration, was produced. Thus in the equation of four dimensions, the whole coefficient of the second term is $a+b+c+d$, and a, b, c, d, were used in the multiplication in obtaining the equation. The coefficient of the third term, of any equation, is made up of members, each of which contains two of the quantities a, b, c, &c. used in the multiplication in obtaining the equation. Thus in the equation of four dimensions, the whole coefficient of the third term is $ab+ac+bc+ad+bd+cd$. And indeed, not only from inspection, but also from considering the manner in which the equations are generated, it is evident that each member of any coefficient has as many of the quantities a, b, c, &c. in it, as there are terms in the equation preceding the term to which the coefficient belongs. Thus each member of the coefficient in the second term of any equation is one quantity only, and only one term precedes the second term. Each member of the coefficient in the third term, of any equation, consists of two quantities, and two terms precede the third, &c.

7. When any equation is multiplied in order to produce the equation next above it, it is evident that the multiplication by x produces a part in the equation to be obtained, which has the same coefficients as the equation multiplied. Thus, multiplying the equation of three dimensions by x we obtain that part of the equation of four dimensions which has the same

coefficients as the cubic: the only effect of this multiplication being the increase of the exponents of x by 1.

8. But when the same equation is multiplied by the quantity adjoined to x by the sign $+$, each term of the product, in order to rank under the same power of x, must be drawn one term back. Thus when the first term of the cubic is multiplied by d, the product must be placed in the second term of the biquadratic. When the second term of the cubic is multiplied by d, the product must be placed in the third term of the biquadratic: and so of others.

9. As the equation last produced is the product of all the compound quantities $x+a$, $x+b$, $x+c$, &c. into one another, and as it was proved in the fourth article that each of the quantities a, b, c, &c. must be found the same number of times in this product, if we can compute the number of times any one of those quantities enters into the coefficient of any term of the last equation, we shall then know how often each of the other enters into the same coefficient: and this may be done with ease, if of the quantities a, b, c, &c. we fix upon that used in the last multiplication. For the last equation, and indeed any other, may be considered as made up of two parts; the first part being the equation immediately before the last multiplied by x, according to the 7th article, and the second part being the same equation multiplied by the quantity adjoined to x by the sign $+$, last used in the multiplication, according to the 8th article. This last used quantity, therefore, never enters into the members of the coefficient of the first of these two parts, but it enters into all the members of the coefficients of the last of them. But that part into which it

does not enter has the same members as the coefficients of the equation immediately before the last, by the 7th article; and when the members of the first part are multiplied by the last used quantity, the product becomes the second part of the whole coefficient above mentioned.

Thus the first part of the cubic equation, by the 7th article is, $\left.\begin{matrix} x^3 + a \\ + b \end{matrix}\right\} x^2 + abx$, and as these coefficients are the same as the coefficients in the quadratic equation, being multiplied by c, and arranged according to the 8th article, we have the coefficients of the second part of the cubic, viz. $c \begin{matrix} + ac \\ + bc \end{matrix} + abc$.

Hence it is evident, that there are as many members in any coefficient, which have the last used quantity in them, as there are members in the coefficient preceding, which have not the same quantity. Thus in the 3d term, in the equation of four dimensions, there are three members of the whole coefficient of x^2 which have d in them, viz. ad, bd, cd, and there are three members of the whole coefficient of x^3 in the second term, which have not d in them, viz. a, b, c. In the fourth term of the same equation, there are three members of the whole coefficient of x, which have d in them, viz. abd, acd, bcd, and there are three members of the whole coefficient of x^2 in the third term which have not d in them, viz. ab, ac, bc. Now as it has been proved that each of the quantities a, b, c, &c. enters the same number of times into the coefficient of the same term, what has here been proved of the last used is applicable to each.

10. From the last article the number of members in the several coefficients of any equation may be determined. For

if we put $s=$ the number of times each quantity is found in a coefficient, $n=$ the number of quantities a, b, c, &c. used in producing the equation, and $p=$ the number of quantities in each member; then as a is found s times in this coefficient, b is found s times in this coefficient, &c. the number of quantities in this coefficient, with their repetitions, will be $s\times n$; and as p expresses the number of quantities requisite for each member, the number of members in the coefficient will be $\frac{sn}{p}$.

Thus, for the sake of illustration, if we limit the above notation to the second term of the equation of five dimensions, $s=1$, as each of the quantities a, b, c, &c. is found once in the whole coefficient of x^4; $p=1$, as each member consists of one quantity, and $n=5$, as a, b, c, d, e are used in producing the equation. Consequently $\frac{sn}{p}=5$. If we limit the above notation to the third term of the same equation, $s=4$, $p=2$, and $n=5$, and therefore $\frac{sn}{p}=10$. If we limit the above notation to the fourth term of the same equation, $s=6$, $p=3$, and $n=5$, and $\frac{sn}{p}=10$. If we limit the above notation to the fifth term of the same equation, $s=4$, $p=4$, and $n=5$, and $\frac{sn}{p}=5$.

11. Using the same notation, we can by the last two articles, calculate the number of members in the next coefficient after that whose number of members is $\frac{sn}{p}$. For as $\frac{sn}{p}$ expresses the number of members in the above mentioned coefficient, and s the number of times each quantity is found in it, $\frac{sn}{p}-s=$ the number of times each is not found in it. By the 9th article therefore, a will be found $\frac{sn}{p}-s$ times, b will be found $\frac{sn}{p}-s$ times, &c. in the next coefficient, and therefore $\overline{\frac{sn}{p}-s}\times n=\frac{sn^2-psn}{p}=$ the number of quantities, with

their repetitions, in it. But as the number of quantities in each member of a coefficient is 1 less than the number in each member of the coefficient next following, each member of the coefficient whose number of members we are now calculating will have in it $p+1$ number of quantities. Consequently $\frac{sn^2-psn}{p\times\overline{p+1}}=\frac{sn}{p}\times\frac{n-p}{p+1}=$ the number of members of the coefficient next after that whose number of members is $\frac{sn}{p}$, as in the last article.

12. It is evident, from the sixth article, that the value of p in the second term of any equation is 1; in the third term of any equation its value is 2; in the fourth term of any equation it is 3, &c. It is also evident that the number of members of the coefficient of the second term of any equation is n; for the whole coefficient is the sum of all the quantities a, b, c, &c. used in producing the equation. It therefore follows that the general expression $\frac{sn}{p}\times\frac{n-p}{p+1}$, obtained in the last article, enables us to ascertain the number of members in the coefficient of any term in an equation. For the number of members of the coefficient in the second term being n, according to the successive values of p the number of members in the third term is $n\,.\frac{n-1}{2}$; in the fourth term it is $n\,.\frac{n-1}{2}.\frac{n-2}{3}$; in the fifth term it is $n\,.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}$; and this regular form may be extended to express the number of members in the coefficient of any term whatever.

13. The binomial theorem, as far as it relates to the raising of integral powers, easily follows from the foregoing articles. For if all the quantities a, b, c, &c. used in the multiplication in the fifth article, be equal to one another, and consequently

each equal to a, each of the members in any coefficient will become a power of a; and, therefore, as the exponent of x in the first term is equal to n, it follows from the sixth and last articles that $\overline{x+a}|^n = x^n + nax^{n-1} + n \cdot \frac{n-1}{2} a^2 x^{n-2} + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot a^3 x^{n-3} + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^4 x^{n-4} +$ &c.

14. If equations be generated from $\overline{x-a} \cdot \overline{x-b} \cdot \overline{x-c} \cdot \overline{x-d}$, &c. the coefficients will be the same, excepting the signs, as those which result from $\overline{x+a} \cdot \overline{x+b} \cdot \overline{x+c} \cdot \overline{x+d}$, &c. in the fifth article; and as minus multiplied into minus gives plus, but minus multiplied into minus multiplied into minus gives minus, the coefficients in equations generated from $\overline{x-a} \cdot \overline{x-b} \cdot \overline{x-c} \cdot \overline{x-d}$, &c. whose members have each an even number of the quantities a, b, c, &c. will have the sign $+$, but coefficients whose members have each an odd number of the quantities a, b, c, &c. will have the sign $-$. And hence it is evident that $\overline{x-a}|^n = x^n - nax^{n-1} + n \cdot \frac{n-1}{2} a^2 x^{n-2} - n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} a^3 x^{n-3} + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^4 x^{n-4} -$ &c.

15. By the general principles of involution $\overline{a+b}|^n = a^n \times \overline{1+\frac{b}{a}}|^n = a^n \times \overline{1+x}|^n$, by putting $x = \frac{b}{a}$. By article 13, $\overline{1+x}|^n = 1 + nx + n \cdot \frac{n-1}{2} x^2 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} x^3 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} x^4 +$ &c. and by the same article $\overline{1+x}|^m = 1 + mx + m \cdot \frac{m-1}{2} x^2 + m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} x^3 + m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \frac{m-3}{4} x^4 +$ &c. But by the general principles of involution, and article 13, $\overline{1+x}|^n \times \overline{1+x}|^m = \overline{1+x}|^{n+m} = 1 + \overline{n+m}\, x + \overline{n+m} \cdot \frac{n+m-1}{2} x^2 + \overline{n+m} \cdot \frac{n+m-1}{2} \cdot \frac{n+m-2}{3} x^3 + \overline{n+m} \cdot \frac{n+m-1}{2} \cdot \frac{n+m-2}{3} \cdot \frac{n+m-3}{4} x^4 +$ &c. when n and m are whole numbers.

Hence it is evident that if the series equal to $\overline{1+x}|^n$ be multiplied by the series equal to $\overline{1+x}|^m$, the product must be equal to the series which is equal to $\overline{1+x}|^{n+m}$. Now the two first mentioned series being multiplied into one another, and the parts being arranged according to the powers of x, the several products will stand as in the following representation.

$$\overline{1+x}|^n = 1 + nx + n \cdot \frac{n-1}{2} x^2 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} x^3 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} x^4 + \text{\&c.}$$

$$\overline{1+x}|^m = 1 + mx + m \cdot \frac{m-1}{2} x^2 + m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} x^3 + m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \frac{m-3}{4} x^4 + \text{\&c.}$$

$$\begin{array}{r} 1 + nx + n \cdot \frac{n-1}{2} x^2 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} x^3 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} x^4 + \text{\&c.} \\ mx + m \cdot nx^2 + m \cdot n \cdot \frac{n-1}{2} x^3 + m \cdot n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} x^4 + \text{\&c.} \\ m \cdot \frac{m-1}{2} x^2 + m \cdot \frac{m-1}{2} \cdot nx^3 + m \cdot \frac{m-1}{2} \cdot n \cdot \frac{n-1}{2} x^4 + \text{\&c.} \\ m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} x^3 + m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot nx^4 + \text{\&c.} \end{array}$$

For the sake of reference hereafter let this be called multiplication A.

Now with respect to the coefficients prefixed to the several powers of x, in the foregoing multiplication, two observations are to be made, by means of which the demonstration of the theorem may be extended to fractional exponents.

In the first place, supposing n and m to be whole numbers, the sum of the coefficients prefixed to any individual power of x, in multiplication A, must be equal to the coefficient prefixed to the same power of x in the binomial series $1 + \overline{n+m}x + \overline{n+m} \cdot \frac{n+m-1}{2} x^2 + \overline{n+m} \cdot \frac{n+m-1}{2} \cdot \frac{n+m-2}{3} x^3 + \overline{n+m} \cdot \frac{n+m-1}{2} \cdot \frac{n+m-2}{3} \cdot \frac{n+m-3}{4} x^4 +$ &c. The certainty of this circumstance rests partly on the 13th article, and partly on a

plain axiom, *viz.* that equals being multiplied by equals the products are equal.

In the second place it is to be observed, that the whole coefficient of any power of x, in the products of multiplication A, may be reduced to the regular binomial form, established in the 13th article. Thus $n \cdot \frac{n-1}{2} + mn + m \cdot \frac{m-1}{2}$, the whole coefficient of x^2, by actual multiplication becomes $\frac{n^2+m^2+2mn-n-m}{2} = \overline{n+m} \cdot \frac{n+m-1}{2}$. Also $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} + mn \cdot \frac{n-1}{2} + m \cdot \frac{m-1}{2} \cdot n + m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3}$, the whole coefficient of x^3, by actual multiplication becomes $\frac{n^3+m^3-3n^2-3m^2+3n^2m}{6} + \frac{3m^2n-6mn+2n+2m}{6} = \overline{n+m} \cdot \frac{n+m-1}{2} \cdot \frac{n+m-2}{3}$. And from the preceding observation it is evident, that we may in the same manner, reduce the whole coefficient of any other power of x, in the products of multiplication A to the regular binomial form.

16. But in proceeding, as above, to change the form of the coefficients prefixed to any power of x, in multiplication A, into the regular binomial form, we are not under the necessity of supposing n and m to be whole numbers. The actual multiplications will end in the same powers of n and m, the same combinations of them, and the same numerals, whether we consider n and m as whole numbers or as fractions.

We are therefore at liberty to suppose n and m to be any two fractions whatever, in the two series multiplied into one another in multiplication A, and the same two fractions will take the place of n and m respectively in the regular binomial series $1 + \overline{n+m}\,x + \overline{n+m} \cdot \frac{n+m-1}{2} x^2 + \overline{n+m} \cdot \frac{n+m-1}{2} \cdot \frac{n+m-2}{3} x^3$

$+\overline{n+m}\,.\,\frac{n+m-1}{2}\,.\,\frac{n+m-2}{3}\,.\,\frac{n+m-3}{4}\,x^4+$ &c. which expresses the product of the two series into one another.

17. If therefore r be any positive whole number we can raise the binomial series $1+\frac{1}{r}x+\frac{1}{r}\,.\,\frac{\frac{1}{r}-1}{2}x^2+\frac{1}{r}\,.\,\frac{\frac{1}{r}-1}{2}\,.\,\frac{\frac{1}{r}-2}{3}x^3+\frac{1}{r}\,.\,\frac{\frac{1}{r}-1}{2}\,.\,\frac{\frac{1}{r}-2}{3}\,.\,\frac{\frac{1}{r}-3}{4}x^4+$ &c. to any proposed power by successive multiplications; or we can express any power of it by supposing the multiplications actually to have been gone through. Thus, calling the last mentioned series the root, if it be multiplied by itself, and if the coefficients in the product be expressed in the regular binomial form, its square will be $1+\frac{2}{r}x+\frac{2}{r}\,.\,\frac{\frac{2}{r}-1}{2}x^2+\frac{2}{r}\,.\,\frac{\frac{2}{r}-1}{2}\,.\,\frac{\frac{2}{r}-2}{3}x^3+\frac{2}{r}\,.\,\frac{\frac{2}{r}-1}{2}\,.\,\frac{\frac{2}{r}-2}{3}\,.\,\frac{\frac{2}{r}-3}{4}x^4+$ &c. Again, if this series be multiplied by the root, and the coefficients in the product be expressed in the regular binomial form, the cube of the root will be $1+\frac{3}{r}x+\frac{3}{r}\,.\,\frac{\frac{3}{r}-1}{2}x^2+\frac{3}{r}\,.\,\frac{\frac{3}{r}-1}{2}\,.\,\frac{\frac{3}{r}-2}{3}x^3+\frac{3}{r}\,.\,\frac{\frac{3}{r}-1}{2}\,.\,\frac{\frac{3}{r}-2}{3}\,.\,\frac{\frac{3}{r}-3}{4}x^4+$ &c. Proceeding thus, by multiplying the last found power by the root, in order to find the next higher power, the nth power of $1+\frac{1}{r}x+\frac{1}{r}\,.\,\frac{\frac{1}{r}-1}{2}x^2+\frac{1}{r}\,.\,\frac{\frac{1}{r}-1}{2}\,.\,\frac{\frac{1}{r}-2}{3}x^3+\frac{1}{r}\,.\,\frac{\frac{1}{r}-1}{2}\,.\,\frac{\frac{1}{r}-2}{3}\,.\,\frac{\frac{1}{r}-3}{4}x^4+$ &c. is $1+\frac{n}{r}x+\frac{n}{r}\,.\,\frac{\frac{n}{r}-1}{2}x^2+\frac{n}{r}\,.\,\frac{\frac{n}{r}-1}{2}\,.\,\frac{\frac{n}{r}-2}{3}x^3+\frac{n}{r}\,.\,\frac{\frac{n}{r}-1}{2}\,.\,\frac{\frac{n}{r}-2}{3}\,.\,\frac{\frac{n}{r}-3}{4}x^4+$ &c.

18. If in the series, which concludes the last article, n be equal to r, the whole series becomes equal to $1+x$. For in this case $\frac{n}{r}=1$, and therefore $\frac{\frac{n}{r}-1}{2}=0$, and consequently every term in the series, after the second, becomes equal to o, or vanishes.

Hence it is evident that the rth root of $1+x$, or, which is the same thing, that $\overline{1+x}|^{\frac{1}{r}}=1+\frac{1}{r}x+\frac{1}{r}\cdot\frac{\frac{1}{r}-1}{2}x^2+\frac{1}{r}\cdot\frac{\frac{1}{r}-1}{2}\cdot\frac{\frac{1}{r}-2}{3}x^3+\frac{1}{r}\cdot\frac{\frac{1}{r}-1}{2}\cdot\frac{\frac{1}{r}-2}{3}\cdot\frac{\frac{1}{r}-3}{4}x^4+$ &c. for this series being raised to the rth power becomes equal to $1+x$.

As by the general principles of involution the nth power of $\overline{1+x}|^{\frac{1}{r}}$ is $\overline{1+x}|^{\frac{n}{r}}$, it therefore follows, from the last observation and the preceding article, that $\overline{1+x}|^{\frac{n}{r}}=1+\frac{n}{r}x+\frac{n}{r}\cdot\frac{\frac{n}{r}-1}{2}x^2+\frac{n}{r}\cdot\frac{\frac{n}{r}-1}{2}\cdot\frac{\frac{n}{r}-2}{3}x^3+\frac{n}{r}\cdot\frac{\frac{n}{r}-1}{2}\cdot\frac{\frac{n}{r}-2}{3}\cdot\frac{\frac{n}{r}-3}{4}x^4+$ &c.

19. By the general principles of involution $\overline{a-b}|^n=a^n\times\overline{1-\frac{b}{a}}|^n=a^n\times\overline{1-x}|^n$, by putting $x=\frac{b}{a}$. By article 14, n being a whole number, $\overline{1-x}|^n=1-nx+n\cdot\frac{n-1}{2}x^2-n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}x^3+n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{n-3}{4}x^4-$ &c. and by the same article, m being a whole number, $\overline{1-x}|^m=1-mx+m\cdot\frac{m-1}{2}x^2-m\cdot\frac{m-1}{2}\cdot\frac{m-2}{3}x^3+m\cdot\frac{m-1}{2}\cdot\frac{m-2}{3}\cdot\frac{m-3}{4}x^4-$ &c. But by the general principles of involution, and article 14, $\overline{1-x}|^n\times\overline{1-x}|^m=\overline{1-x}|^{n+m}$

$$=1-\overline{n+m}x+\overline{n+m}\cdot\frac{n+m-1}{2}x^2-\overline{n+m}\cdot\frac{n+m-1}{2}\cdot\frac{n+m-2}{3}x^3$$
$$+\overline{n+m}\cdot\frac{n+m-1}{2}\cdot\frac{n+m-2}{3}\cdot\frac{n+m-3}{4}x^4-\&c.$$

Hence it is evident that if the series equal to $\overline{1-x}|^n$ be multiplied by the series equal to $\overline{1-x}|^m$, the product must be equal to the series, which is equal to $\overline{1-x}|^{n+m}$. Now the two first mentioned series being multiplied into one another, and the parts being arranged according to the powers of x, the several products will stand as in the following representation.

$$\overline{1-x}|^n=1-nx+n\cdot\frac{n-1}{2}x^2-n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}x^3+n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{n-3}{4}x^4-\&c.$$
$$\overline{1-x}|^m=1-mx+m\cdot\frac{m-1}{2}x^2-m\cdot\frac{m-1}{2}\cdot\frac{m-2}{3}x^3+m\cdot\frac{m-1}{2}\cdot\frac{m-2}{3}\cdot\frac{m-3}{4}x^4-\&c.$$

$$\begin{array}{llll}
1-nx+n\cdot\frac{n-1}{2}x^2 & -n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}x^3 & +n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{n-3}{4}x^4 & -\&c.\\
-mx+m\cdot nx^2 & -m\cdot n\cdot\frac{n-1}{2}x^3 & +m\cdot n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}x^4 & -\&c.\\
\quad m\cdot\frac{m-1}{2}\cdot x^2 & -m\cdot\frac{m-1}{2}\cdot nx^3 & +m\cdot\frac{m-1}{2}\cdot n\cdot\frac{n-1}{2}x^4 & -\&c.\\
 & -m\cdot\frac{m-1}{2}\cdot\frac{m-2}{3}x^3 & +m\cdot\frac{m-1}{2}\cdot\frac{m-2}{3}\cdot nx^4 & -\&c.
\end{array}$$

For the sake of reference hereafter let this be called multiplication B.

Now for the same reasons as are stated in the 15th and 16th articles, the whole coefficient prefixed to any power of x in multiplication B, must be equal to the coefficient prefixed to the same power of x in the series $1-\overline{m+n}x+\overline{m+n}\cdot\frac{m+n-1}{2}x^2-\overline{m+n}\cdot\frac{m+n-1}{2}\cdot\frac{m+n-2}{3}x^3+\overline{m+n}\cdot\frac{m+n-1}{2}\cdot\frac{m+n-2}{3}\cdot\frac{m+n-3}{4}x^4-\&c.$; and we are also at liberty to suppose n and m to be any two fractions whatever, in the series multiplied into one another, and consequently in the series expressing their product.

Proceeding therefore as in the 17th and 18th articles, and using the same notation, $\overline{1-x}|^{\frac{1}{r}} = 1 - \frac{1}{r}x + \frac{1}{r} \cdot \frac{\frac{1}{r}-1}{2}x^2 - \frac{1}{r} \cdot \frac{\frac{1}{r}-1}{2} \cdot \frac{\frac{1}{r}-2}{3}x^3 + \frac{1}{r} \cdot \frac{\frac{1}{r}-1}{2} \cdot \frac{\frac{1}{r}-2}{3} \cdot \frac{\frac{1}{r}-3}{4}x^4 -$ &c.

Also $\overline{1-x}|^{\frac{n}{r}} = 1 - \frac{n}{r}x + \frac{n}{r} \cdot \frac{\frac{n}{r}-1}{2}x^2 - \frac{n}{r} \cdot \frac{\frac{n}{r}-1}{2} \cdot \frac{\frac{n}{r}-2}{3}x^3 + \frac{n}{r} \cdot \frac{\frac{n}{r}-1}{2} \cdot \frac{\frac{n}{r}-2}{3} \cdot \frac{\frac{n}{r}-3}{4}x^4 -$ &c.

20. It is easily proved, by means of the 15th and 16th articles, that

$$\frac{\overline{1+x}|^m \quad 1+mx+m \cdot \frac{m-1}{2}x^2+m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3}x^3+m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \frac{m-3}{4}x^4+ \text{ \&c.}}{\overline{1+x}|^n \quad 1+nx+n \cdot \frac{n-1}{2}x^2+n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3}x^3+n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4}x^4+ \text{ \&c.}} \text{ or}$$

is equal to the series $1+\overline{m-n}x+\overline{m-n} \cdot \frac{m-n-1}{2}x^2+\overline{m-n} \cdot \frac{m-n-1}{2} \cdot \frac{m-n-2}{3}x^3+\overline{m-n} \cdot \frac{m-n-1}{2} \cdot \frac{m-n-2}{3} \cdot \frac{m-n-3}{4}x^4+$ &c whether m and n be whole numbers or fractions. For v being equal to $m-n$, this last series becomes $1+vx+v \cdot \frac{v-1}{2}x^2+v \cdot \frac{v-1}{2} \cdot \frac{v-2}{3}x^3+v \cdot \frac{v-1}{2} \cdot \frac{v-2}{3} \cdot \frac{v-3}{4}x^4+$ &c.; and this series being multiplied by $1+nx+n \cdot \frac{n-1}{2}x^2+n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3}x^3+n \cdot \frac{n-1}{2} \cdot \frac{n-1}{3} \cdot \frac{n-3}{4}x^4+$ &c. the series expressing their product, by the 15th and 16th articles, is $1+\overline{v+n}x+\overline{v+n} \cdot \frac{v+n-1}{2}x^2+\overline{v+n} \cdot \frac{v+n-1}{2} \cdot \frac{v+n-1}{3}x^3+\overline{v+n} \cdot \frac{v+n-1}{2} \cdot \frac{v+n-2}{3} \cdot \frac{v+n-3}{4}x^4+$ &c. But as v is equal to $m-n$, this last series is equal to $1+mx+m \cdot \frac{m-1}{2}x^2+m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3}x^3+m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \frac{m-3}{4}x^4+$ &c.

Hence it is evident that $\frac{\overline{1+x}|^m}{\overline{1+x}|^n}$ is equal to $1+\overline{m-n}x+\overline{m-n}$

$\frac{m-n-3}{4}x^4+$ &c.; and as this equation holds in every possible value of m, and as, by the general principles of involution $\overline{1+x}|^{0}$ is equal to 1, when m is equal to o then $\frac{1}{\overline{1+x}|^{n}}$, or $\overline{1+x}|^{-n}=1-nx-n.\frac{-n-1}{2}x^2-n.\frac{-n-1}{2}.\frac{-n-2}{3}x^3-n.\frac{-n-1}{2}.\frac{-n-2}{3}.\frac{-n-3}{4}x^4-$ &c.

According to the form of the binomial series, the whole of the second, fourth, sixth, &c. terms in the last series consist of an odd number of negative parts multiplied into one another, and therefore each of these terms becomes a negative quantity. But the whole of the third, fifth, seventh, &c. terms, consist of an even number of negative parts multiplied into one another, and therefore each of these terms becomes a positive quantity. Consequently, $\overline{1+x}|^{-n}=1-nx+n.\frac{n+1}{2}x^2-n.\frac{n+1}{2}.\frac{n+2}{3}x^3+n.\frac{n+1}{2}.\frac{n+2}{3}.\frac{n+3}{4}x^4-$ &c.

21. By the 19th article we are enabled to prove that

$$\text{or}\ \frac{\overline{1-x}|^{m}\quad 1+m.-x+m.\frac{m-1}{2}x^2+m.\frac{m-1}{2}.\frac{m-2}{3}.-x^3+m.\frac{m-1}{2}.\frac{m-2}{3}.\frac{m-3}{4}x^4+\text{\&c.}}{\overline{1-x}|^{n}\quad 1+n.-x+n.\frac{n-1}{2}x^2+n.\frac{n-1}{2}.\frac{n-2}{3}.-x^3+n.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}x^4+\text{\&c.}}$$

is equal to the series $1+\overline{m-n}.-x+\overline{m-n}.\frac{m-n-1}{2}x^2+\overline{m-n}.\frac{m-n-1}{2}.\frac{m-n-2}{3}.-x^3+\overline{m-n}.\frac{m-n-1}{2}.\frac{m-n-2}{3}.\frac{m-n-3}{4}x^4+$ &c. For, as in the preceding article, if this last series be multiplied by $1+n.-x+n.\frac{n-1}{2}x^2+n.\frac{n-1}{2}.\frac{n-2}{3}.-x^3+n.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}x^4+$ &c. the series expressing the product will be $1+m.-x+m.\frac{m-1}{2}x^2+m.\frac{m-1}{2}.\frac{m-2}{3}.-x^3+m.\frac{m-1}{2}$

$\frac{m-2}{3} \cdot \frac{m-3}{4} x^4 +$ &c. Consequently as $\frac{\overline{1-x}|}{\overline{1-x}|^n} = 1 + \overline{m-n} \cdot -x + \overline{m-n} \cdot \frac{m-n-1}{2} x^2 + \overline{m-n} \cdot \frac{m-n-1}{2} \cdot \frac{m-n-2}{3} \cdot -x^3 + \overline{m-n} \cdot \frac{m-n-1}{2} \cdot \frac{m-n-2}{3} \cdot \frac{m-n-3}{4} x^4 +$ &c. in every possible value of m, it follows that when m is equal to 0, then $\frac{1}{\overline{1-x}|^n}$ or $\overline{1-x}|^{-n}$ $= 1 - n \cdot -x - n \cdot \frac{-n-1}{2} x^2 - n \cdot \frac{-n-1}{2} \cdot \frac{-n-2}{3} \cdot -x^3 - n \cdot \frac{-n-1}{2} \cdot \frac{-n-2}{3} \cdot \frac{-n-3}{4} x^4 -$ &c.

The form of this series, however, may be changed into one more convenient. For the whole of the second, fourth, sixth, &c. terms consist of an even number of negative parts multiplied into one another, and therefore each of these terms becomes a positive quantity. And as the coefficients of the third, fifth, seventh, &c. terms consist of an even number of negative parts multiplied into one another, and as in these terms the powers of x are positive, each of these terms becomes a positive quantity. Consequently $\overline{1-x}|^{-n} = 1 + nx + n \cdot \frac{n+1}{2} x^2 + n \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} x^3 + n \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} \cdot \frac{n+3}{4} x^4 +$ &c.

Every particular necessary for the establishment of the binomial theorem has now been proved. I therefore proceed to conclude the subject, by shewing that each of the four forms, in which the theorem may be expressed, immediately follows from the preceding articles, and the general principles of involution. In each of them n is to be considered either as a whole number or fraction.

22. By article 18, $\overline{1+x}|^n = 1 + nx + n \cdot \frac{n-1}{2} x^2 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} x^3 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} x^4 +$ &c. But if $\frac{b}{a}$ be equal to x, then

$a^n \times \overline{1+\frac{b}{a}}|^n = \overline{a+b}|^n$, by the general principles of involution; and therefore $\overline{a+b}|^n = a^n \times : 1 + n\frac{b}{a} + n.\frac{n-1}{2}\frac{b^2}{a^2} + n.\frac{n-1}{2}.\frac{n-2}{3}.\frac{b^3}{a^3} + n.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}\frac{b^4}{a^4} + \&c. = a^n + nba^{n-1} + n.\frac{n-1}{2}b^2a^{n-2} + n.\frac{n-1}{2}.\frac{n-2}{3}b^3a^{n-3} + n.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}b^4a^{n-4} + \&c.$

By article 19, $\overline{1-x}|^n = 1 - nx + n.\frac{n-1}{2}x^2 - n.\frac{n-1}{2}.\frac{n-2}{3}x^3 + n.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}x^4 - \&c.$ and therefore as before, if $\frac{b}{a}$ be equal to x, $\overline{a-b}|^n = a^n - nba^{n-1} + n.\frac{n-1}{2}ba^{n-2} - n.\frac{n-1}{2}.\frac{n-2}{3}b^3a^{n-3} + n.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}b^4a^{n-4} - \&c.$

By article 20, $\overline{1+x}|^{-n} = 1 - nx + n.\frac{n+1}{2}x^2 - n.\frac{n+1}{2}.\frac{n-2}{3}x^3 + n.\frac{n+1}{2}.\frac{n+2}{3}.\frac{n+3}{4}x^4 - \&c.$ and therefore if $\frac{b}{a}$ be equal to x, $\overline{1+\frac{b}{a}}|^{-n} = 1 - n\frac{b}{a} + n.\frac{n+1}{2}\frac{b^2}{a^2} - n.\frac{n+1}{2}.\frac{n+2}{3}\frac{b^3}{a^3} + n.\frac{n+1}{2}.\frac{n+2}{3}.\frac{n+3}{4}\frac{b^4}{a^4} - \&c.$ But by the general principles of involution $\frac{1}{a^n \times \overline{1+\frac{b}{a}}|^n} = a^{-n} \times \overline{1+\frac{b}{a}}|^{-n} = \overline{a+b}|^{-n}$; and therefore $\overline{a+b}|^{-n}$ $a^{-n} - nba^{-n-1} + n.\frac{n+1}{2}b^2a^{-n-2} - n.\frac{n+1}{2}.\frac{n+2}{3}b^3a^{-n-3} + n.\frac{n+1}{2}.\frac{n+2}{3}.\frac{n+3}{4}b^4a^{-n-4} - \&c.$

By article 21, $\overline{1-x}|^{-n} = 1 + nx + n.\frac{n+1}{2}x^2 + n.\frac{n+1}{2}.\frac{n+2}{3}x^3 + n.\frac{n+1}{2}.\frac{n+2}{3}.\frac{n+3}{4}x^4 + \&c.$; and therefore if $\frac{b}{a}$ be equal to x, $\overline{1-\frac{b}{a}}|^{-n} = 1 + n\frac{b}{a} + n.\frac{n+1}{2}\frac{b^2}{a^2} + n.\frac{n+1}{2}.\frac{n+2}{3}\frac{b^3}{a^3} + n.\frac{n+1}{2}.\frac{n+2}{3}.\frac{n+3}{4}\frac{b^4}{a^4} + \&c.$ But by the general principles of involution $\frac{1}{a^n \times \overline{1-\frac{b}{a}}|^n} = a^{-n} \times \overline{1-\frac{b}{a}}|^{-n} = \overline{a-b}|^{-n}$; and therefore $\overline{a-b}|^{-n} =$

$$a^{-n}+nba^{-n-1}+n\,.\,\frac{n+1}{2}\,b^2\,a^{-n-2}+n\,.\,\frac{n+1}{2}\,.\,\frac{n+2}{3}\,b^3\,a^{-n-3}+n\,.\,\frac{n+1}{2}\,.\,\frac{n+2}{3}\,.\,\frac{n+3}{4}\,b^4\,a^{-n-4}+\ \&c.$$

The four forms expressed in this article include the whole of the binomial theorem.

XVI. *New Method of computing Logarithms. By* Thomas Manning, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read June 5, 1806.

If there already existed as full and extensive logarithmic tables as will ever be wanted, and of whose accuracy we were absolutely certain, and if the evidence for that accuracy could remain unimpaired throughout all ages, then any new method of computing logarithms would be totally superfluous so far as concerns the formation of tables, and could only be valuable indirectly, inasmuch as it might shew some curious and new views of mathematical truth. But this kind of evidence is not in the nature of human affairs. Whatever is recorded is no otherwise believed than on the evidence of testimony; and such evidence weakens by the lapse of time, even while the original record remains; and it weakens on a twofold account, if the record must from time to time be replaced by copies. Nor is this destruction of evidence arising from the uncertainty of the copy's being accurately taken, any where greater than in the case of copied numbers.

It is useful then to contrive new and easy methods for computing not only new tables, but even those we already have. It is useful to contrive methods by which any part of a table may be verified independently of the rest; for by

examining parts taken at random, we may in some cases satisfy ourselves of its accuracy, as well as by examining the whole.*

Among the various methods of computing logarithms, none, that I know of, possesses this advantage of forming them with tolerable ease independently of each other by means of a few easy bases. This desideratum, I trust, the following method will supply, while at the same time it is peculiarly easy of application, requiring no division, multiplication, or extraction of roots, and has its relative advantages highly increased by increasing the number of decimal places to which the computation is carried.

The chief part of the working consists in merely setting down a number under itself removed one or more places to the right, and subtracting, and repeating this operation; and consequently is very little liable to mistake. Moreover, from the commodious manner in which the work stands, it may be revised with extreme rapidity. It may be performed after a few minutes instruction by any one who is competent to subtract. It is as easy for large numbers as for small; and on an average about 27 subtractions will furnish a logarithm acccurately to 10 places of decimals. In general $9 \times \frac{n+1}{2}$ subtractions will be accurate to $2n$ places of decimals.

In computing hyperbolic logarithms by this method it is necessary to have previously establised the h. logs. of $\frac{10}{9}$, $\frac{100}{99}$, $\frac{1000}{999}$, &c. of 2 and of 10.

* For example, we may wish to know whether the editor of a table has been careless. We examine detached portions here and there to a certain extent; if we find no errors, we have a moral certainty that the editor was careful, and consequently a moral certainty that the edition is accurate.

With respect to the logs. of $\frac{10}{9}$, $\frac{100}{99}$, $\frac{1000}{999}$, &c. their computation is very easy,* they being the respective sums of the series

$$\frac{1}{10} + \frac{1}{2} \times \frac{1}{\overline{10}|^2} + \frac{1}{3} \times \frac{1}{\overline{10}|^3} + \frac{1}{4} \times \frac{1}{\overline{10}|^4} + \&c.$$

$$\frac{1}{100} + \frac{1}{2} \times \frac{1}{\overline{100^2}|} + \frac{1}{3} \times \frac{1}{\overline{100}|^3} + \frac{1}{4} \times \frac{1}{\overline{100}|^4} + \&c.$$

$$\frac{1}{1000} + \frac{1}{2} \times \frac{1}{\overline{1000}|^2} + \frac{1}{3} \times \frac{1}{\overline{1000^3}|} + \&c.$$

&c.

of which series each is more easily summed than the preceding.

With respect to the logarithms of 2 and 10 there are, it is well known, various ways of computing them, and the time requisite depends greatly on the practical habits of the calculator. Among other ways, they may be computed by the method given in this Paper, and with what degree of expedition, may be seen by the examples to the rules, where they are both of them worked.†

* Ex. H. 1. $\frac{1000}{999} = \frac{1}{1000} + \frac{1}{2} \times \frac{1}{\overline{1000}|^2} + \frac{1}{3} \frac{1}{\overline{1000}|^3} + \&c.$

1st term = ,001

2d term = ,0000005

3d term = ,000000000333

Sum = ,001000500333, which is true to the last place of decimals.

† It may be seen there that the logarithm of 10 by this method requires very little work. The log. of 2 may also be computed from the log. of 10 as follows.

$2^{10} = 1024 = 1000 \times \left(1 + \frac{24}{1000}\right)$ therefore log. $2^{10} = 3 \times$ log. 10 + log. $\left(1 + \frac{24}{1000}\right) = 3 \times$ log. 10 + $\left(\frac{24}{1000} - \frac{1}{2} \frac{\overline{24}|^2}{\overline{1000}|^2} + \frac{1}{3} \times \frac{\overline{24}|^3}{\overline{1000}|^3} - \&c.\right)$

TABLE, *containing the hyperbolic Logarithms of* $\frac{10}{9}$, $\frac{100}{99}$, $\frac{1000}{999}$, *&c. of* 2 *and of* 10; *together with the Reciprocal of the last or Modulus of the common Logarithms.*

H. l. of	$\frac{10}{9}$	$=,105360515655$
h. l.	$\frac{100}{99}$	$=,010050335853$
h. l.	$\frac{1000}{999}$	$=,001000500333$
h. l.	$\frac{10000}{9999}$	$=,000100005\ldots$
h. l.	$\frac{100000}{99999}$	$=,00001000005$
h. l.	$\frac{1000000}{999999}$	$=,000001$
h. l.	$\frac{10000000}{9999999}$	$=,0000001$
h. l.	$\frac{100000000}{99999999}$	$=,00000001$

$$
\begin{array}{r}
1000)24(.024 \\
2 \\
\hline
48 \\
12 \\
2\times\overline{1000}|^{2}\ )576(.000288 \\
2 \\
\hline
1152 \\
12 \\
3\times\overline{1000}|^{3}\ )13824(.000004608 \\
2 \\
\hline
27648 \\
12 \\
4\times\overline{1000}|^{4}\ )331776(.000000082944 \\
2 \\
\hline
663452 \\
12 \\
5\times\overline{1000}|^{5}\ )7962624(.000000001592
\end{array}
$$

Sum of odd terms	,024004609592
—— even ——	,000288082944
Difference	,023716526648
$3\times$ log. 10	6,907755279648
Log. 2^{10} - -	6,931471806296. True to the 10th figure.

and so on; the unit receding regularly to the right.

h. l. 2 = ,693147180637

h. l. 10 = 2,302585093217

$\frac{1}{\text{h. l. 10}}$ = ,434294481861.

Certain Multiples of the above Numbers; viz. all those required in computing Logarithms by the subjoined Rules, and which are not evident upon Inspection.

Multiples of the h. logs. of $\frac{10}{9}$, $\frac{100}{99}$, &c.		Multiples of h. l. 2, h. l. 10, and $\frac{1}{\text{h. l. 10.}}$	
		Of h. l. 2.	
Double h. l. of $\frac{10}{9}$	= ,210721031310	Double	=1,386294361274
triple	= ,316081546965	triple	=2,079441541911
quadruple	= ,421442062620	quadple.	=2,772588722548
5ple.	= ,526802578275	5ple.	=3,465735903185
6ple.	= ,632163093930	6ple.	=4,158883083822
7ple.	= ,737523609585	7ple.	4,852030264459
8ple.	= ,842884125240	8ple.	5,545177445096
9ple.	= ,948244640895	9ple.	6,238324625733
		Of h. l. 10	
Double h. l. $\frac{100}{99}$	= ,020100671706	Double	=4,605170186434
triple	,030151007559	triple	=6,907755279651
quadruple	,040201343412	quadple.	9,210340372868
5ple.	,050251679265	5ple.	11,512925466085
6ple.	,060302015118	6ple.	13,815510559302
7ple.	,070352350971	7ple.	16,118095652519
8ple.	,080402686824	8ple.	18,420680745736
9ple.	,090453022677	9ple.	20,723265838953
		Of $\frac{}{\text{h. l. 10}}$.	
Double h. l. $\frac{1000}{999}$	= ,002001000666	Double	= ,868588963722
triple	= ,003001500999	triple	=1,302883445583
quadruple	= ,004002001332	quadple.	=1,737177927444
5ple.	= ,005002501665	5ple.	=2,171472409305
6ple.	= ,006003001998	6ple.	=2,605766891166
7ple.	= ,007003502331	7ple.	=3,040061373027
8ple.	= ,008004002664	8ple.	=3,474355854888
9ple.	= ,009004502997	9ple.	=3,908650336749

I. *To find the hyperbolic Logarithm of any Number not exceeding* 2.

RULE. Set the number under itself, to be subtracted from itself, but removed so many places to the right as shall be necessary to make the remainder greater than 1; subtract. Proceed in the same manner with the remainder, and so on till the remainder becomes 1 followed by $\frac{1}{2}$ as many cyphers as the number of decimal places you work to; suppose at the end of the operation you find that you have removed *one* place to the right and subtracted b times; *two* places, c times; three places, d times, &c.; then $b \times$ h. l. $\frac{10}{9} + c \times$ h. l. $\frac{100}{99} +$ $d \times$ h. l. $\frac{1000}{999}$ + &c. + decimal part of the last remainder = h. l. sought.

And these numbers are collected together out of the Table; for b, c, d, &c. can none of them ever exceed 9.

Ex. 1. *To find the h. l. of* 2 *to* 10 *Places of Decimals.*

2.	$6 \times \text{h. l.} \frac{10}{9} = .632163093930$
.2	
1.8	$6 \times \text{h. l.} \frac{100}{99} = .060302015118$
.18	
1.62	$6 \times \text{h. l.} \frac{10000}{9999} = .00060003\ldots$
162	
1.458	$8 \times \text{h. l.} \frac{\overline{10}\vert^{5}}{99999} = .0000800004\ldots$
.1458	
1.3122	$2 \times \text{h. l.} \frac{\overline{10}\vert^{6}}{999999} = .000002\ldots\ldots$
13122	
1.18098	decim. last rem. $= .000000041189$
118098	
1.062882 6	h. l. 2 = 693147180637
1062882	
1.05225318	
105225318	
1.0417306482	
10417306482	
1.031313341718	
103131334171	
1.0210002083009	
102100020830	
1.0107902062179	
101079020621	
1.0006823042558 ......6.	

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1.0006823042558
1000682304

1.0005822360254
1000582236

1.0004821778018
1000482177

1.0003821295841
1000382129

1.0002820913712
1000282091

1.0001820631621
1000182063

1.0000820449558 6
100008204

1.0000720441354
100007204

1.0000620434150
100006204

1.0000520427946
100005204

1.0000420422742
100004204

1.0000320418538
100003204

1.0000220415334
100002204

1.0000120413130
100001204

1.0000020411926 8
10000020

1.0000010411906
10000010

1.0000000411896 Last remainder.

Ex. II. *To find the h. l. of* 1.25.

1.25
125
1.125
.1125
1.0125 2
10125
1.002375 1
1002375
1.001372625
1001372625
1.000371252375 2
1000371252
1.0002712152498
1000271215
1.0001711881283
1000171188
1.0000711710095 3
100007117
1.0000611702978
100006117
1.0000511696861
100005116
1.0000411691745
100041116
1.0000311687629
100003116
1.0000211684513
100002116
1.0000111682397
100001116
1.0000011681281 7
10000011
1.0000001681270 last remainder.

$2 \times$ h. l. $\frac{10}{9}$	....	,210721031310
$1 \times$ h. l. $\frac{100}{99}$	....	,010050335853
$2 \times$ h. l. $\frac{1000}{999}$	....	,002001000666
$3 \times$ h. l. $\frac{\overline{10}\|^{4}}{9999}$	....	,000300015000
$7 \times$ h. l. $\frac{\overline{10}\|^{5}}{99999}$	....	,000070000350
$1 \times$ h. l. $\frac{\overline{10}\|^{6}}{999999}$	....	,000001 ......
decimal of last remainder	....	,000000168127
log. required	....	,223143551306

II. *To find the h. l. of any Number, whole or mixt.*

Rule. Reduce the given number (if necessary) to a whole or mixt number less than 2, by setting the decimal point after the first significant figure, or if the given number be 10 or a power of 10 after the first 0; and then dividing by 2 (if necessary) till the integral part is 1.*

Find the h. l. of this reduced number by the last rule, and add to it or subtract from it as many times the h. l. of 10 as the decimal point was removed places to the left or right; also add to it as many times the h. l. of 2 as there were divisions by 2. The sum is the h. l. required.

Ex. iii. *To find the h. l. of* 10.

$\frac{10}{2^3} = 1.25$, whose h. log. is found in the last example to be

	,223143551306
h. l. 2^3 =	2,079441541911
h. l. 10	2,302585093217

Ex. iv. *To find the h. l. of* 5548748 *to 6 Places of Decimals.*

4)5.548748	
1.387187	
1387187	
1.2484683	
12484683	
1.12362147	
112362147	
1.011259323 . . . 3	
10112593	
1.001146730 . . . 1	
1001146	
1.000145584	last remainder.

$3 \times$ h. l. $\frac{10}{9}$	=	,316081546
$1 \times$ h. l. $\frac{100}{99}$	=	,010050335
$1 \times$ h. l. $\frac{1000}{999}$	=	,001000500
decimal of last remainder	=	,000145584
h. log. 1.387187	=	,327277965
$6 \times$ h. l. 10	=	13,815510559
$2 \times$ h. l. 2	=	1,386294361
log. required		15,529082885

* Three divisions by 2 will always suffice.

Ex. v. *To find the h. l. of 7 to 5 Places of Decimals.*

	4)7.
	1.75
	175
	1.575
	1575
	1.4175
	14175
	1.27575
	127575
	1.148175
	1148175
5	1.0333575
	1033357
	1.02302393
	1023023
	1.01279370
	1012793
3	1.00266577
	100266
	1.00166311
	100166
last rem.	1.00066145

$5 \times$ h. l. $\frac{10}{9}$ ...	,5268025
$3 \times$ h. l. $\frac{100}{99}$	,0301510
$2 \times$ h. l. $\frac{1000}{999}$	,0020010
decim^l of last rem^r ...	,0006614
$2 \times$ h. l. 2 ...	1,3862943
log. 7 =	1,9459102

From the small number of subtractions that have been necessary, the log. of 7 must be correct to 6 places of decimals.

III. *To find the common Logarithm of any Number.*

Rule. Find the h. l. by the above rule, and multiply $\frac{1}{h.\,l.\,10}$ into it.

Otherwise. Proceed by art. 2, omitting what concerns the

h. l. 10. Multiply into $\frac{1}{\text{h. l. } 10}$, and add or subtract as many units as the decimal point was removed to the left or right.

Note. The multiplication of a number by $\frac{1}{\text{h. l. } 10}$ is very expeditiously performed by means of the Table of multiples of $\frac{1}{\text{h. l. } 10}$.

The demonstration of the above rules is obvious. Setting the figures of a number one place to the right is dividing that number by 10; 2 places, by 100; 3 places, by 1000; and so on. And subtracting a number, so placed, from the number itself is subtracting a 10th, a 100th, a 1000th, &c. (in the respective cases) of the number from itself; and consequently the remainders are (respectively) $\frac{9}{10}$ths, $\frac{99}{100}$ths, $\frac{999}{1000}$ths, &c. of the numbers subtracted from. Let b, c, d, &c. denote as in the rule; then the original number $= \overline{\frac{10}{9}}\Big|^{b} \times \overline{\frac{100}{99}}\Big|^{c} \times \overline{\frac{1000}{999}}\Big|^{d} \times$ &c. $\times$ last remainder. Therefore the log. of the original number $= b \times \log. \frac{10}{9} + c \times \log. \frac{100}{99} + d \times \log. \frac{1000}{999} +$ &c. $+$ log. of last remainder. Now the last remainder being unity followed by a certain number of decimal cyphers, its correct hyp. log., as far as twice that number of places, is (as is well known) the decimal part itself of that remainder. Hence the rule is manifest.

A similar method, by addition only, by means of the ready computed logarithms of $\frac{11}{10}$, $\frac{101}{100}$, $\frac{1001}{1000}$, &c. might, in some cases, be used with advantage. Let N denote the given number, consisting of unity and a decimal whose h. l. is sought; and let P denote any number less than N, and whose h. l. is previously known. Set P under itself removed one

or more places to the right; add; and proceed with the sum in the same manner, till you have obtained a number, $N \pm a$, the difference between which and N shall be inconsiderable. Let b, c, d, &c. denote as in the rule.

Then $P \times \overline{\tfrac{11}{10}}|^{b} \times \overline{\tfrac{101}{100}}|^{c} \times \overline{\tfrac{1001}{1000}}|^{d} \times$ &c. $= N \pm a$.

Therefore $\log. \overline{N \pm a} = \log. P + b \times \log. \tfrac{11}{10} + c \times \log. \tfrac{101}{100} + d \times \log. \tfrac{1001}{1000} +$ &c., which call B.

And $\log. N = \log. \frac{N}{N \pm a} + \log. \overline{N \pm a} = \log. \frac{N}{N \pm a} + B$.

Now we may either carry the operation so far that $\log. \frac{N}{N \pm a}$ may be neglected, or we may actually divide N by $N - a$, or $N + a$ by N (according as the sign is $-$ or $+$) and add or subtract the quotient from B.

Various artifices may be occasionally used to shorten the computation both in the method of subtraction, and in this of addition; and the two may sometimes be advantageously combined together.

It should be observed that, in setting down the numbers, the last figure set down ought to be increased by unity when the figure immediately following in the neglected part exceeds 4.

EXAMPLE *of the Method of Addition.* *To find the h. l. of* 2.

1.1	*Continued.*
11	1.991863
1.21	1992
121	1.993855
1.331	1994
1331	1.995849
1.4641	1996
14641	1.997845
1.61051	1998
161051	1.999843
1.771561	199
177156	2.000042
1.948717	
19487	
1.968204	
19682	
1.987886	
1988	
1.989874	
1989*	
1,991863	

From this operation it appears, that

$$\frac{11}{10}\times\overline{\frac{11}{10}}\Big|^6\times\overline{\frac{101}{100}}\Big|^2\times\overline{\frac{1001}{1000}}\Big|^6\times\frac{10001}{10000}=2.000042.$$

Consequently, $7\times$ h.l. $\frac{11}{10}+2\times$ h.l. $\frac{101}{100}+6\times$ h.l. $\frac{1001}{1000}+$ h. l. $\frac{10001}{10000}=$ h. l. 2,000042.

$=$ h. l. 2 $+$ h. l. $\frac{2.000042}{2}=$ h. l. 2 $+$ h. l. (1.000021) $=$ h. l. 2 $+$ 000021.

The method by subtraction has many advantages over this

* Instead of this number 1989, it would be more correct to set down 1990, because the first figure of the neglected part, 874, exceeds 4.

by addition. It is more simple, and being more completely mechanical, may be confided to the most unskilful without danger of error. And though addition be an easier operation than subtraction, yet the greater facility arising from this circumstance will not be found sufficient to balance these and other advantages.

XVII. *Description of the Mineral Bason in the Counties of Monmouth, Glamorgan, Brecon, Carmarthen, and Pembroke. By Mr.* Edward Martin. *Communicated by the Right Hon.* C. F. Greville, *F. R. S.*

Read May 22, 1806.

1. THE irregular oval line, delineated on the annexed map (Plate XIV.) shows nearly the inner edge of a limestone bason, in which all the strata of coal and iron ore (commonly called Iron Stone) in South Wales are deposited; the length of this bason is upwards of 100 miles, and the average breadth in the counties of Monmouth, Glamorgan, Carmarthen, and part of Brecon, is from 18 to 20 miles, and in Pembrokeshire only from 3 to 5 miles.

2. On the north side of a line, that may be drawn in an east and west direction, ranging nearly through the middle of this bason, all the strata rise gradually northward; and on the south side of this line they rise southward, till they come to the surface, except at the east end, which is in the vicinity of Pontipool, where they rise eastward.

3. The depths from the surface to the various strata of coal and iron ore depend upon their respective local situations.

4. The deepest part of the bason is between Neath, in Glamorganshire, and Llanelly, in Carmarthenshire; the uppermost stratum of coal here does not extend a mile in a north

and south direction, and not many miles in an east and west direction, and its utmost depth is not above 50 or 60 fathoms.

5. The next stratum of coal, and those likewise beneath it, lie deeper and expand still longer and wider, and the lowest which are attended by parallel strata of iron ore, of which there are in some situations about 16 accompanied by irregular balls or lumps of iron ore, occupy the whole space between Llanmaddock Hill, near the entrance of Burry river, to Llanbidie, from the Mumbles to Cribbath, from Newton Down to Penderryn, from Castle Coch to Castle Morlais, and from Risca to Llangattock, and in length on the south side of the bason from Pontypool through Risca, Tinkwood, Llantrissent, Margam, Swansea Bay, and Cline Wood, to Llanmaddock Hill, and on the north side through Blaenafon, Ebbw, Sirhowy, Merthyr, Aberdare, Aberpergwm, Glyntowy, Llandibie, and the Great Mountain, to Pembrey Hill, near Llanelly in Carmarthenshire, and their depths are at the centre range of strata from 6 to 700 fathoms.

6. The strata of coal and iron ore running from Pembrey Hill, through Carmarthen Bay and Pembrokeshire to St. Bride's Bay, are only a continuation of those in the counties of Glamorgan and Carmarthen, which lie next to and parallel with the north side of the bason, all the remaining strata rising southward; and the middle ranges on the north side of the bason, are lost between where they meet the sea near Llanmaddock Hill and the south side of Pembrey Hill, in their course towards Pembrokeshire, in consequence of a contraction of the sides of the mineral bason, or rather by its becoming shallower; for in Pembrokeshire none of the strata of coal or iron ore lie above 80 or 100 fathoms deep, consequently

all those which do not lie above 5 or 600 fathoms in Glamorganshire and Carmarthenshire have not reached this county, by reason of the bason not being of sufficient depth and width to hold them.

7. The strata of coal at the east end of the bason running from Pontypool to Blaenafon and Clydach, and on the north side from thence to Nanty Glo, Ebbw, Beaufort, Sirhowy, Tredegar, Romney, Dowlais, Penderryn, Plymouth, Cyfarthfa, Abernant, Aberdare and Hurwain Furnaces and Iron Works, are of a cokeing quality, and from thence the whole strata of coal to St. Bride's Bay alter in their quality, to what is called Stone Coal, (the large of which has hitherto been used for the purposes of drying malt and hops, and the small, which is called Culm, for burning of limestone); the several strata of coal from Pontypool, on the south side of the bason, through Risca, Llantrissent, Margam, and Cline Wood, to Burry River, Llanelly, and the south side of Pembrey Hill, are principally of a bituminous or binding quality.

8. Notwithstanding the principal strata of coal in Glamorganshire, lie from 5 fathoms to 6 or 700 fathoms deep, still it has not been necessary to pursue these strata deeper than about 80 fathoms.

9. The veins of coal and iron ore, in the vicinity of most of the iron works in Monmouthshire and Glamorganshire are drained and worked by levels or horizontal drifts, which opportunity is given by the deep valleys which generally run in a north and south direction, intersecting the range of coal and iron ore, which run in an east and west direction, under the high mountains, and thereby serving as main drains, so that the collier or miner here gets at the treasures of the

earth, without going to the expence and labour of sinking deep pits, and erecting powerful fire-engines. However, in process of time, in situations where the coal and iron ore that are above the level of these natural drains, become exhausted, it will be found necessary to sink shallow pits, and erect fire-engines for the draining and working of the coal and iron ore, and at a future period, pits of greater depths, must be sunk for the same purposes.

10. There are 12 veins or strata of coal in this mineral depository, from 3 feet to 9 feet thick each; which together make $70\frac{1}{2}$ feet: and there are 11 more, from 18 inches to 3 feet, which make $24\frac{1}{4}$ feet, making in all 95 feet; besides a number of smaller veins from 12 to 18 inches, and from 6 to 12 inches in thickness, not calculated upon.

11. By taking the average length and breadth of the foregoing different strata of coal, the amount is about 1000 square miles, containing 95 feet of coal in 23 distinct strata, which will produce in the common way of working 100,000 tons *per* acre, or 64,000,000 tons *per* square mile.

12. If the whole extent of this mineral country was an even plain, the border or outbreak of each stratum would appear regular and true; but owing to the interposition of hills and valleys, the edges of the strata, if nicely measured and planned, would seem indented and uneven, yet in many instances the due range is totally thrown out of course, in consequence of knots, dikes, or faults.

13. These faults, or irregularities are not confined to the edges of the strata, but they take grand ranges, through the interior of the bason, generally in a north and south direction, and often throw the whole of the strata, for hundreds of acres

together, 40, 60, 80, or 100 fathoms, up or down, and still there is seldom any superficial appearance, that indicates a disjunction, for the largest faults frequently lie under even surfaces.

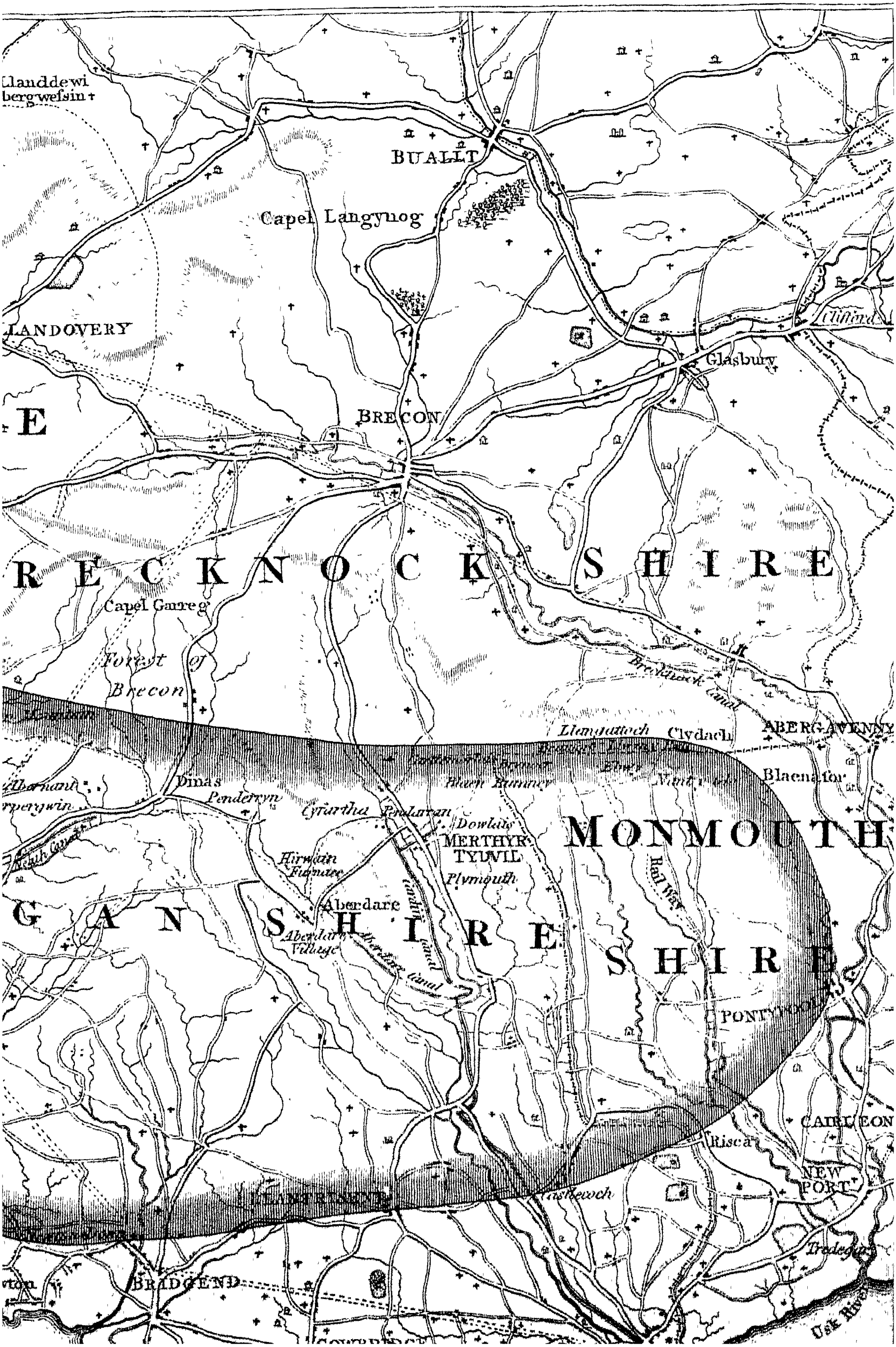
14. As every stratum rises regularly from its base to the surface, and is frequently visible and bare, in precipices and deep dingles, and often discovered where the earth or soil is shallow in trenching, or in forming high roads, and by reason of the whole of the country within this boundary being so perforated by pits, and so intersected by the various operations of art and nature, it is not probable that any vein of coal, iron ore, or other stratum remains undiscovered in this mineral bason.

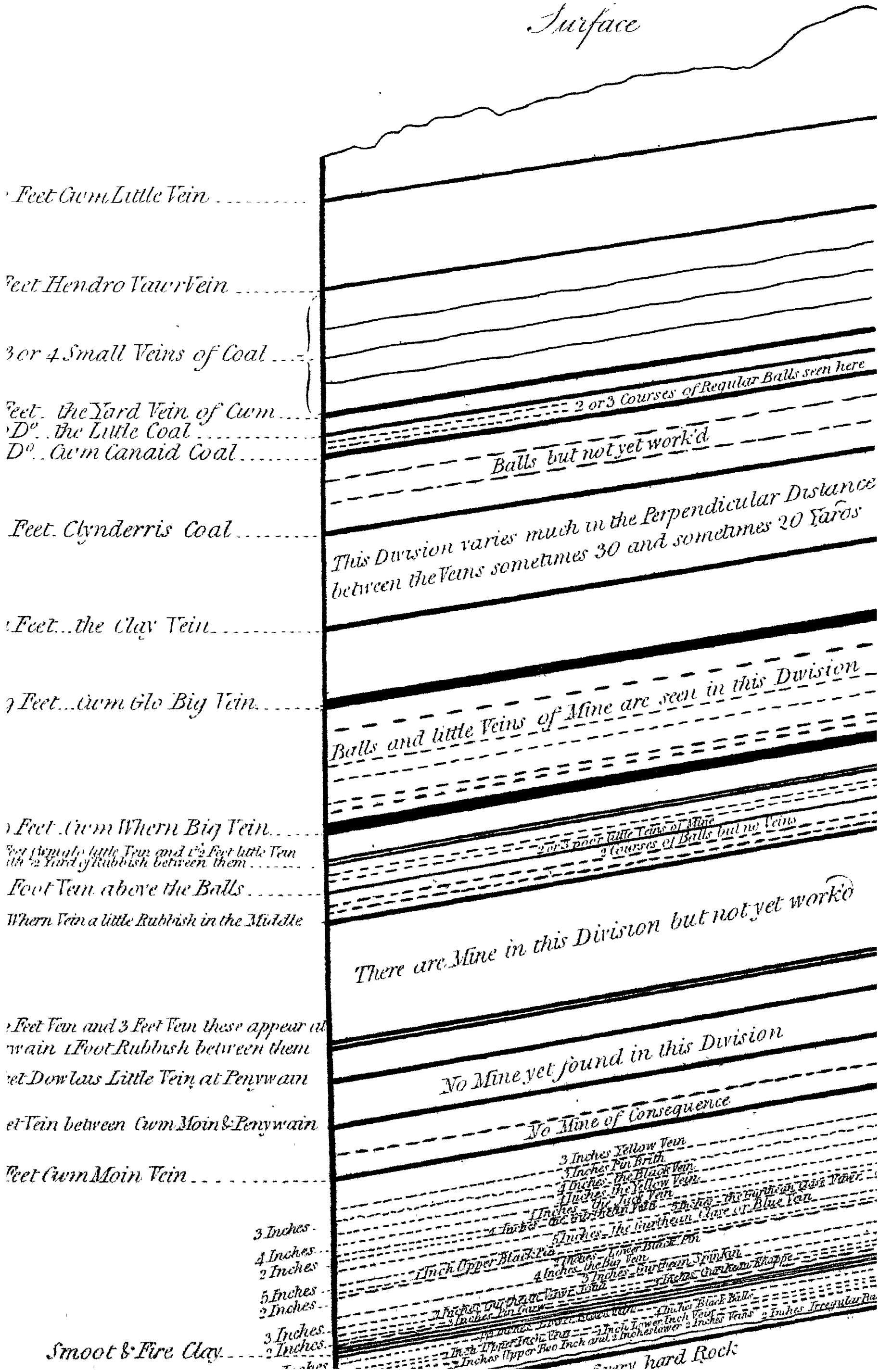
15. Glamorganshire engrosses far the greatest portion of coal and iron ore, Monmouthshire the next in point of quantity, Carmarthenshire the next, Pembrokeshire the next, and Brecknockshire possesses the least.

16. The strata of coal and iron ore in the last named county, which are the lowest in the bason, break out northward, and only take place in the three following distinct spots, *viz.* 1st. From Turch River (which is the boundary between Lord CAWDOR and CHARLES MORGAN, Esq.) across the river Tawe and the Drin Mountain to the great forest of Brecon. 2d. A corner of ground from Blaen Romney to the north of Brynoer. 3d. Another spot, from Rhyd Ebbw and Beaufort Iron Works, through Llwyn y Pwll, near Tavern Maed Sur, to where it joins Lord ABERGAVENNY'S mineral property.

17. *Note.* A principal fault is observable at Cribbath, where the beds or strata of the limestone stand erect: another,


CARDIGAN
CARDIGAN S
Bayvill
Llantood
KILGERRAN
NEWPORT
Eglwyswrw
NEWCASTLE in Emlyn
Kilgwyn
Pont Cynnon
Llanichloidog
Penboir
Brescelly M.
Twy Bach yr Rhos
E SHIRE
Llanvernach
C A E R M A R T H
Clarbeston
CAERMARTHEN
Wiston
Colby
Robestone wathen
Whitland
St Clare
Slebitch
Picton
NARBETH
Tavern Spite
Llanstephan
Greswell
Isels
Marros
LLAUGHARNE
KIDWELLY
Sanders foot
Pembrey hill
C A E R M A R T H E N
Ivy Tower
Carew
TENBY
B A Y
Llane
Penally
Lealston Bay
St Margarets I.
Stackpole Court
Fresh Water East
Caldy I.
Burry River
Llanmadlock hill
Worms Head
Port Inon Point
Orwich Point


Llanddewi
bergwefsin
BUALLT
Capel Langynog
LANDOVERY
Glasbury
Clifford
BRECON
BRECKNOCK SHIRE
Capel Garreg
Forest of
Brecon
Breknock Canal
Llangattoch
Clydach
ABERGAVENNY
Blaenafor
Dinas
Penderryn
Cyfartha
Penderran
Blaen Rumney
Dowlais
MERTHYR
TYDVIL
Plymouth
Hirwain
Furnace
Aberdare
Aberdare
Village
Aberdare Canal
MONMOUTH
SHIRE
Half Way
PONTYPOOL
CAIRLEON
Risca
NEW
PORT
Castlecoch
LLANTRISENT
Tredegar
BRIDGEND
COWBRIDGE
Usk River



Surface
Feet Cwm Little Vein
Feet Hendro Vawr Vein
3 or 4 Small Veins of Coal
Feet. the Yard Vein of Cwm
Dº. the Little Coal
Dº. Cwm Canaid Coal
2 or 3 Courses of Regular Balls seen here
Balls but not yet work'd
Feet. Clynderris Coal
This Division varies much in the Perpendicular Distance between the Veins sometimes 30 and sometimes 20 Yards
Feet...the Clay Vein
Feet...Cwm Glo Big Vein
Balls and little Veins of Mine are seen in this Division
Feet Cwm Whern Big Vein
2 or 3 poor little Veins of Mine
2 Courses of Balls but no Veins
Foot Vein above the Balls
Whern Vein a little Rubbish in the Middle
There are Mine in this Division but not yet work'd
Feet Vein and 3 Feet Vein these appear at
wain 1 Foot Rubbish between them
No Mine yet found in this Division
et Dowlais Little Vein at Penywain
et Vein between Cwm Moin & Penywain
No Mine of Consequence
Feet Cwm Moin Vein
3 Inches Yellow Vein
3 Inches Pin Brith
3 Inches
4 Inches
2 Inches
5 Inches
2 Inches
3 Inches
2 Inches
1 Inch Upper Black Pin
4 Inches the Big Vein
Smoot & Fire Clay
hard Rock

of considerable magnitude, lies between Ystradvellte and Penderryn, where all the strata on the north side of the bason are moved many hundreds of yards southward (as at Dinas).

18. *Note.* The limestone appears to the surface all along the boundary line in the counties of Monmouth, Glamorgan, Carmarthen, Brecon, and no doubt can be entertained of its due range from Newton across Swansea Bay to the Mumbles, and from Llanmaddock Hill across Carmarthen Bay to Tenby. In Pembrokeshire it appears to the surface on the south side of the bason, at Tenby, Ivy Tower, Cochelard, Bit Church, Williamston, Lawrinny, Cord, Canta, and Johnston; and on the north side of the bason, at Templeton, Picton, Harriston, and Persfield; yet it certainly forms an underground connection from point to point.

XVIII. *Observations on the Permanency of the Variation of the Compass at Jamaica. In a Letter from Mr.* James Robertson *to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S. &c.*

Read June 12, 1806.

SIR,

As any improvement, or discovery in the arts and sciences, will, I am persuaded, experience your favourable reception, I have the honour of submitting to your consideration a discovery I have made on a subject, the state of which can only be ascertained by observations made from time to time, as it is not regulated by any known law of nature: I mean the variation of the magnetical needle.

This discovery may not only excite others to make, and repeat, observations in different parts of the globe, but, by causing this changeable quality to be better understood, may contribute to the benefit of navigation, and commerce, as well as to the advancement of a more particular knowledge of the subject.

It has hitherto been considered, that the variation of the magnetical needle is not fixed in any particular place, but is constantly varying, in a greater or a less degree, in all parts of the world. I have discovered an exception to this supposed general property of variation; and, as it may be, perhaps, the first that has been made, it will require proportionally strong

proof to establish it. This, I flatter myself, I am able to effect, to the certainty of demonstration itself: but, in doing so, I am under the necessity of being more tedious than I could wish, in order to describe fully the data, on which the inference is founded.

I resided at Jamaica, as a King's Surveyor of Land, upwards of 20 years. Disputes at law about boundaries of lands are there decided by ejectments, in the Supreme Court of Judicature, by the evidence and diagrams of King's surveyors of land. This is different from the practice in England, because the manner in which grants of land from the Crown are made, in the two countries is different. In Jamaica, to every grant of land a diagram thereof is annexed to the patent. This diagram is delineated from an actual survey of the land to be granted, having a meriodional line, according to the magnetical needle, by which the survey was made, laid down in it. No notice is taken of the true meridian. The boundary lines of the land granted are marked on earth, (as it is denominated,) by cutting notches on the trees between which the line is run through the woods. These trees being mostly of hard timber, the notches will be discernible for 30 years, or more. By repeated re-surveys these lines are kept up: and, when the cultivation, on both sides, renders it necessary to fell the marked trees, (which can only be done by mutual consent, it being otherwise death by the law,) logwood fences are planted in the lines dividing the properties thus cultivated: and many of these fences have been regularly repaired, and kept up, to the present time. Lands were granted from the Crown soon after the Restoration, in 1660; and every succeeding year the number of patents increased,

The old estates have been often re-surveyed, and plans of them made, and usually annexed to deeds of conveyance, or mortgage, which must be enrolled, within a limited time, in the office of the Secretary of the Island; where, also, all the patents, and diagrams annexed to them, are recorded. In all disputes at law about boundary lines, where the keeping up of the old marked lines on earth has been neglected, surveyors are appointed to make actual re-surveys of all the old marked lines on earth, (preserved in the manner before mentioned,) and to extract from the Secretary of the Island's office, correct copies of all such diagrams annexed to patents, and to deeds of conveyance, or mortgage, of lands in the neighbourhood where the disputed boundary is, as they may think necessary for the investigation thereof. They then compare the lines, and meridians, of these original diagrams with those in their diagrams delineated from their own re-surveys recently made; when it is always expected that the lines, and meridians, of the former will coincide with those of the latter. It is evident that this coincidence could not happen if any variation of the magnetical needle had taken place in the intermediate time elapsed between the making of the first, and of the last, survey. My business being very extensive, I was frequently applied to in disputes at law about boundary lines, and I had, besides, abundance of opportunities, on other surveys, to ascertain this fact satisfactorily. From all which I have discovered that the courses of the lines, and meridians, delineated on the original diagrams annexed to patents, from 1660, downwards to the present time, and of the re-survey diagrams thereof, annexed to deeds, coincide with, and are parallel to, the lines and meridians delineated

on the new diagrams from recent surveys made by the magnetical needle, of the same original marked lines on earth, preserved as before described); so that whatever course is laid down for the line on the diagram annexed to the patent, (and let it be supposed, for example, to be north and south, or east and west,) upon setting the compass in the old marked line on earth, and directing the sights north and south, or east and west, according to the magnetical needle, the said marked line on earth, originally run by the magnetical needle 130 or 140 years ago, has been found by me to be exactly in the line, or direction with that of the compass; consequently no alteration of the variation could have taken place during the whole, or any part, of that period of time in Jamaica.

To this it may not be unacceptable to subjoin a short history of the practice of surveying in Jamaica, from the Restoration to the present time, in order to obviate any doubt that might arise, whether there be not a possibility of the quantity of the magnetical variation having been ascertained, and allowed for, in the first diagrams annexed to patents; and whether the variation of $6\frac{1}{2}$ degrees east, which corresponds with the magnetical needle now, might not then, have have agreed with the true meridian.

The variation of the compass was first observed by COLUMBUS, in his first voyage across the Atlantic, in the year 1492; and seemed to threaten that the laws of nature were altered in an unknown ocean. It is evident, however, that COLUMBUS was not able to ascertain the quantity of variation; for if he had ascertained it, the danger he was in would have been diminished, if not entirely removed. His discovery, therefore, must have been, simply, the deflection of the magnetical

needle from the true meridian, without knowing the quantity thereof.

From this period down to the year 1700, when Dr. HALLEY published his "Theory of the Variation of the Compass," no observations, ascertaining the quantity of variation, in the West Indies, were, I believe, published. He was the first that made any in South America, and these were chiefly applicable to the coast of Brasil. With his theory was published "A new and correct Chart of the whole World, shewing the "Variations of the Compass, &c. as they were found in 1700, "by Direction of Capt. EDM. HALLEY." By this chart the variation, at Jamaica, appears to have been the same as it is at present. His theory could have been known but to few; nor do any observations, in the West Indies, appear to have been made for many years after its publication. Indeed I know of none till very lately, and these only in a few charts. But, however extensive its publicity might have been, it could have had no influence in directing the surveys, in Jamaica, that preceded it by 30 or 40 years.

The ascertaining of the true meridian, and, consequently, of the magnetical variation from it, requires more scientifical, as well as practical, knowledge, than is often to be met with even at this time; but, 130 or 140 years back, it was entirely confined to a very few individuals. The magnetical needle was then the only guide and rule to go by, both at sea and at land, and, generally, without any reference being had to the true meridian.

Had the first surveyors ascertained the quantity of variation, and allowed for it, in delineating their diagrams that were annexed to the earliest patents in Jamaica, they would

have mentioned the same in such diagrams; otherwise it could only tend to mislead, not to direct. The same system of surveying would, and must, by law, have been continued; for, (as was stated above,) the number of grants has been annually increased; and the uninterrupted practice of surveying, which was always daily increasing in proportion to the extending cultivation and settlement of the island, could not admit of any change, without a new law having been made by the legislature for that purpose: and then such a change must have been recorded with the laws of the island, and with those that regulated the conduct of surveyors. No surveyor, nor other person, could have been ignorant of such a change having taken place. Since even the difference of one degree in running a line is very considerable; but that of six would have totally changed all property, deranged all boundaries, thrown woodlands into plantations, and *vice versâ:* and, consequently, would have been so palpable and injurious as to have demanded legislative interference and correction. But no such change has ever happened, nor has the most remote idea of it ever been entertained. On the contrary, the magnetical meridian, in all disputes at law about boundary lines, is and always has been the only criterion by which the surveyors, the court, and the jury, decide.

From the year 1700, when Dr. HALLEY's theory was published, it is very easy to trace down the practice of surveying in Jamaica, as well as up to its commencement. When I arrived in that island, upwards of 25 years since, I became acquainted with the oldest surveyors there, who had practised from 30 to 40 years. They had the original papers, field notes, and diagrams of their predecessors, up to the

dates of the first surveys. Many of these original papers, field notes, and diagrams are now in my possession; from which the practice of surveying, taking field notes, and delineating them on diagrams, is clearly shown.

Jamaica was early divided into counties and parishes, the boundary lines whereof were defined by the legislature, and the lines of many marked on earth. In the county of Surrey, the line, dividing the parishes of Portland and St. George, is a north and south line, by law, and was marked on earth according to the magnetical needle. It continues in the same direction. In the county of Cornwall, the dividing line between the parishes of St. James and Trelawney continues a north and south line, on earth, as it was first run by the magnetical needle. This will be evident on the inspection of my maps of Jamaica, lately published. It became necessary, in giving the island its true position on the globe, to ascertain its latitude and longitude; and also the true meridian, with the quantity of the variation of the magnetical meridian from it. But I have applied these meridians differently in the maps of the counties, and in that of the island. In the former, in which the situation on the globe is not given, the magnetical is laid down as the principal meridian; because all surveys of every other description, as well as those of the boundary lines of counties and parishes, are regulated by it; and the true meridian is introduced only to show the variation; but, in the latter, in which its place on the globe is fixed, as to latitude and longitude, the true meridian becomes the principal one; and the magnetical meridian shows the quantity of variation from it, and regulates the surveys, and the relative situation of places, as in the county maps.

When Sir HENRY MOORE, (who was considered a great surveyor,) was governor of Jamaica, about the year 1760, maps of that island were constructed, under his immediate direction, by Mr. CRASKELL, the island engineer, and Mr. SIMPSON, both eminent surveyors. But, in these maps, the magnetical meridian only is represented. Neither the magnetical variation nor the true meridian is mentioned: the island's place on the globe, as to latitude and longitude, is not given. In short, the true meridian has never been noticed, nor the quantity of variation ascertained, nor the variation even mentioned, nor the latitude and longitude, observed by any *surveyor or engineer* in Jamaica, but myself.

Although the discovery of the variation's not varying, in Jamaica, is established on the clearest evidence without the aid of other data, yet it is highly gratifying to find Dr. HALLEY, as it were, confirming it to the minutest accuracy, as will appear from the recital of the following observations of Mr. LONG, in his History of Jamaica.

" The variations of the magnetical needle were observed " by Dr. HALLEY to be very small, near the equator. I have " seen no account of them for this island, that can be relied " upon; but, if observations should be faithfully made here, " they would probably confirm his opinion. According to " MOUNTAIN's chart, constructed in the year 1700, from Dr. " HALLEY's tables, the variation at Port Royal then was about " $6\frac{1}{2}$ degrees east. But, as in most parts of the world it is " found to be continually either increasing or decreasing, so " we may reasonably conclude, that it may have altered in " both respects very much during this long interval that has " passed since the constructing of the chart."

The magnetical variation, ascertained by me, and laid down in my maps of Jamaica, is $6\frac{1}{2}$ degrees east.

I leave to others, better qualified than I am, to enquire, and to point out, what improvements natural philosophy may derive from this discovery; which I hope may be an acquisition to science.

I am afraid I have been too prolix. But the importance of the subject, and my desire to remove even the shadow of any doubt that might be suggested, will, I trust, be admitted as my apology.

I have the honour to be, &c.

JAMES ROBERTSON.

XIX. *Observations on the Camel's Stomach respecting the Water it contains, and the Reservoirs, in which that Fluid is inclosed; with an Account of some Peculiarities in the Urine. By* Everard Home, *Esq. F.R.S.*

Read June 12, 1806.

THE Board of Curators of the Museum belonging to the Royal College of Surgeons, formed of seven Members of the Court of Assistants, have from their first appointment embraced every opportunity of augmenting the HUNTERIAN Collection: and in December, 1805, hearing that a camel in a dying state was to be sold, purchased it with a view of illustrating the anatomy of that animal. They appointed Mr. LONG, (their Chairman,) Mr. CLINE, Teacher of Anatomy, with Sir WILLIAM BLIZARD and myself, the two Professors of Anatomy and Surgery to the College, a Committee for that purpose.

As Professor of Comparative Anatomy I was directed to examine the peculiarities of the stomach, and to make a report on that subject. This report appeared to the Board of Curators to contain some facts, which had not before been ascertained; and it is at their desire that the present communication is made.

The camel, the subject of the following observations, was a female, brought from Arabia; 28 years old, and said to have

been 20 years in England, and 12 years in the possession of the person, from whom the Board of Curators purchased it. Its height was seven feet from the ground to the tip of the anterior hump.

In December, 1805, it came under the care of the Committee. At that time it was so weak as hardly to be able to stand. It got up with difficulty, and almost immediately kneeled down again. By being kept warm, and well fed, it recovered so as to be able to walk, but was exceedingly infirm on its feet: and moved with a very slow pace. It drank regularly every second day six gallons of water, and occasionally seven and a half; but refused to drink in the intervening period. It took the water by large mouthfuls, and slowly, till it had done. The quantity of food it daily consumed was one peck of oats, one of chaff, and one-third of a truss of hay. Some of the urine was saved, and sent to Mr. HATCHETT for the purpose of having it analyzed: his account of its component parts is contained in a report annexed to this Paper.

In the beginning of February, 1806, it began to shed its coat. Towards the end of March the wind became extremely cold, and the animal suffered so much from it, that it lost its strength, refused its food, and drank only a small quantity of water at a time.

In this state it was thought advisable to put an end to so miserable an existence: and it suggested itself to the Committee that if this was done soon after the animal had drank a quantity of water, the real state of the stomach might be ascertained.

On the 1st of April, by giving the animal hay mixed with

a little salt it was induced to drink, at two different times in the course of two hours three gallons of water: not having taken any the three preceding days, or shewn the least disposition to do so. Three hours after this, its head was fixed to a beam, so as to prevent the body from falling to the ground, after it was dead, and in this situation it was pithed by Mr. CLINE, junior, assisted by Mr. BRODIE and Mr. CLIFT. This operation was performed with a narrow double-edged poniard passed in between the skull and first vertebra of the neck; in this way the medulla oblongata was divided, and the animal instantaneously deprived of sensibility. In the common mode of pithing cattle the medulla spinalis only is cut through, and the head remains alive, which renders it the most cruel mode of killing animals that could be invented.*

The animal was kept suspended, that the viscera might remain in their natural state, and in two hours the cavities of the chest, and abdomen were laid open, in the presence of all the Members of the Committee, and Mr. CHANDLER, a Member of the Board of Curators.

The first stomach was the only part of the contents of the abdomen, which appeared in view. The smooth portion of the paunch was on the left side, and on the right towards the chest was a cellular structure, in which it was evident to the feel there was air, but no part of the solid food, with which the general cavity was distended. On the lower posterior part towards the pelvis there was another portion made up of cells, larger and more extensive than that, which was

* See Dr. DUGARD's experiments, published in the Board of Agriculture's Report for Shropshire, by JOSEPH PLUMLEY, M. A. p. 246.

anterior. On pressing on this part a fluctuation of its contents could be distinctly perceived. A trocar with the canula was plunged into the most prominent of the cells, and on withdrawing it there passed through the canula 12 oz. of water of a yellow colour, but unmixed with any solid matter. This fact having been ascertained, the first stomach was laid open, on the left side, at a distance from the cellular structure, and the solid contents were all removed. While this was doing some water flowed out of the cells, and some out of the second stomach, but the greater part was retained. That in the second stomach was nearly pure: while the other was muddy, and of a yellow colour, tinged by the contents of the first stomach. On examining the cellular structure no part of the solid food had entered it, nor was there any in the second stomach: those cavities having their orifices so constructed as to prevent the solid food from entering, even when empty.

On measuring the capacities of these different reservoirs in the dead body, they were as follows:

The anterior cells of the first stomach were capable of containing one quart of water, when poured into them. The posterior cells three quarts. One of the largest cells held two ounces and a half, and the second stomach four quarts. This, however, must be considered as much short of what those cavities can contain in the living animal, since there are large muscles covering the bottom of the cellular structure, to force out the water, which must have been contracted immediately after death and by that means had diminished the cavities.

By this examination it was proved, in the most satisfactory manner, that the camel when it drinks, conducts the water in

a pure state into the second stomach, that part of it is retained there, and the rest runs over into the cellular structure of the first, acquiring a yellow colour in its course.

This confirms the account given by M. Buffon in his examination of the camel's stomach, as well as that of other travellers, who state that when a camel dies in the desart, they open the stomach, and take out the water, which is contained in it, to quench their thirst.

That the second stomach in the camel contained water, had been generally asserted, but by what means the water was kept separate from the food had never been explained, nor had any other part been discovered, by which the common offices of a second stomach could be performed. On these grounds Mr. Hunter did not give credit to the assertion, but considered the second stomach of the camel to correspond in its use with that of other ruminants, as appears from his observations on this subject stated by Dr. Russell, in his history of Aleppo.

The difference of opinion on this subject led me to examine accurately the structure of the stomachs of the camel, and of those ruminants which have horns, so as to determine, if possible, the peculiar offices belonging to their different cavities.

The most satisfactory mode of communicating the result of this inquiry will be first to describe the different stomachs of the bullock, and then those of the camel, and afterwards to point out the peculiarities, by which this animal is enabled to go a longer time without drink than others, and thereby fitted to live in those sandy desarts of which it is the natural inhabitant. The relative position of the parts is described while the animal

was suspended, as that was the state in which the different stomachs could be most accurately examined, without disturbing their contents.

When the first stomach of the bullock is laid open by a longitudinal incision on the left side of the œsophagus, and the solid contents are removed, which in general are very dry, that cavity appears to be made up of two large compartments, separated from each other by two transverse bands of considerable thickness, and the second stomach forms a pouch or lesser compartment, on the anterior part of it, rather to the right of the œsophagus, so that the first and second stomach are both included in one general cavity, and lined with a cuticle.

The œsophagus appears to open into the first stomach, but on each side of its termination there is a muscular ridge, projecting from the coats of the first stomach, so as to form a channel into the second stomach.

These muscular bands however do not terminate there, but are continued on to the orifice of the third stomach, in which they are lost.

When these parts are examined, it is evident that the food can pass readily from the œsophagus, either into the general cavity of the first stomach or into the second, which last is peculiarly fitted by its situation, and the muscular power of its coats both to throw up its contents into the mouth, and to receive a supply from the general cavity of the first stomach at the will of the animal.

It was ascertained by examining the stomachs of several bullocks immediately after they were knocked down, that the second stomach contained the same kind of food as the first, only more moist; it must therefore be considered as a shelf

from which the food may be regurgitated along the canal, continued from the œsophagus. There is indeed no other mode by which this can be effected, since it is hardly possible for the animal to separate small portions from the suface of the mass of dry food in the first stomach, and force it up into the mouth.

It was also found that when the bullock had been four days without water before it was killed, which is by no means uncommon, the food in the second stomach was very moist, while that in the first was very dry; and when the animal was killed 24 hours after having had water, by making an opening into the second stomach before the other parts were disturbed, nearly a quart of water ran out of it, little mixed with solid food. The man of the slaughter-house also mentioned, upon being asked where the water was met with, that it was always found in the honeycomb'd bag. The water must be received into this stomach while the animal is drinking, for it could not afterwards be conveyed there from the first, as it would naturally drain through the food and remain at the bottom of its cavity.

The second stomach, by receiving the water, is enabled to have its contents always in a proper state of moisture, to admit of its being readily thrown up into the mouth for rumination, which appears to be the true office of this stomach, and not to receive the food after that process has been gone through, as is very generally believed, for in that case the cud would be mixed and lost in the general contents of this cavity, instead of being forwarded to the true digesting stomach.

When the food is swallowed the second time, the orifice of the third stomach is brought forwards by the muscular bands,

which terminate in it, so as to oppose the end of the œsophagus, and receive the morsel, without the smallest risk of its dropping into the second stomach.

The third stomach of the bullock is a cavity, in the form of a crescent, containing 24 septa, 7 inches broad, about 23, 4 inches broad, and about 48 of $1\frac{1}{4}$ inch, at their broadest part. These are ranged in the following order. One broad one, with one of the narrowest next it; then a narrow one, with one of the narrowest next it; then a broad one, and so on. The septa are very thin membranes, covered with a cuticle, and have their origin in the orifice leading from the œsophagus, so that whatever passes into the cavity must fall between these septa, and describe three-fourths of a circle, before it can arrive at the orifice leading to the true stomach, which is so near the other, that the distance between them does not exceed three inches: and therefore the direct line from the termination of the œsophagus to the orifice of the fourth stomach is only of that length. While the young calf is fed on milk, that liquor, which does not require to be ruminated, is conveyed directly to the fourth stomach, not passing between the plicæ of the third; and afterwards the solid food is directed into that cavity, by the plicæ being separated from each other.

The food found in the third stomach is of the consistence of thick paste: and is met with in the form of flattened pellets, distributed between the different septa.

When this cavity is opened, it emits an odour of a very unpleasant kind, arising from the process, which the food undergoes in it.

The third stomach opens into the fourth by a projecting

valvular orifice, and the cuticular lining terminates exactly on the edge of this valve, covering only that half of it, which belongs to the third.

The fourth or true digesting stomach is about 2 feet 9 inches long: its internal membrane has 18 plicæ beginning at its orifice, (9 on each side,) 4 inches broad. They are continued down for about 22 inches, increasing to a great degree its internal surface: beyond these the internal membrane is thrown into rugæ, which follow a very serpentine direction, and close to the pylorus there is a glandular projection, one end of which is opposed to the orifice, and closes it up, when in a collapsed state.

These appearances will be better explained by the drawings (Plates XV. and XVI.) than by verbal description.

The camel's stomach anteriorly forms one large bag, but when laid open is found to be divided into two compartments on its posterior part, by a strong ridge which passes down from the right side of the orifice of the œsophagus in a longitudinal direction. This ridge forms one side of a groove that leads to the orifice of the second stomach, and is continued on beyond that part, becoming one boundary to the cellular structure met with in that situation. From this ridge eight strong muscular bands go off at right angles, and afterwards form curved lines till they are insensibly lost in the coats of the stomach. These are at equal distances from each other, and being intersected in a regular way by transverse muscular septa, form the cells. This cellular structure is in the left compartment of the stomach, and there is another of a more superficial kind on the right, placed in exactly the opposite direction, made up of 21 smaller rows of cells, but entirely unconnected with the great ridge. The appearance these parts put on, and

their relative situation, will be distinctly seen in the annexed drawing (Plate XVII.). On the left side of the termination of the œsophagus a broad muscular band has its origin, from the coats of the first stomach, and passes down in the form of a fold parallel to the great ridge, till it enters the orifice of the second stomach, which gives it another direction. It is continued along the upper edge of that cavity, and terminates within the orifice of a small bag, which may be termed the third stomach.

This band on one side, and the great ridge on the other, form a canal, which leads from the œsophagus down to the cellular structure in the lower part of the first stomach.

The orifice of the second stomach, when this muscle is not in action, is nearly shut, and at right angles to the side of the first. Its cavity is a pendulous bag, in which there are 12 rows of cells, formed by as many strong muscular bands passing in a transverse direction, and intersected by weaker muscular bands so as to form the orifices of the cells. Above these cells, between them and the muscle, which passes along the upper part of this stomach, is a smooth surface extending from the orifice of this stomach to the termination in the third.

From this account, it is evident that the second stomach neither receives the solid food in the first instance, as in the bullock, nor does it afterwards pass into its cavity or cellular structure.

The food first passes into the general cavity of the first stomach, and that portion of it, which lies in the recess immediately below the entrance of the œsophagus under which the cells are situated is kept moist, and is readily returned into

the mouth, along the groove formed for that purpose, by the action of the strong muscle, which surrounds this part of the stomach, so that the cellular portion of the first stomach in the camel performs the same office as the second in the ruminants with horns. While the camel is drinking, the action of the muscular band opens the orifice of the second stomach, at the same time that it directs the water into it: and when the cells of that cavity are full, the rest runs off into the cellular structure of the first stomach immediately below, and afterwards into the general cavity; it would appear that camels, when accustomed to go journeys in which they are kept for an unusual number of days without water, acquire the power of dilating the cells, so as to make them contain a more than ordinary quantity as a supply for their journey, at least such is the account given by those who have been in Egypt. When the cud has been chewed it has to pass along the upper part of the second stomach before it can reach the third. How this is effected without its falling into the cellular portion, could not from any inspection of dried specimens be ascertained; and it was in this state only that Mr. HUNTER saw the internal structure of the camel's stomach; but when the recent stomach is accurately examined, the mode in which this is managed becomes very obvious. At the time that the cud is to pass from the mouth the muscular band contracts with so much force, that it not only opens the orifice of the second stomach, but acting on the mouth of the third, brings it forwards into the second, by which means the muscular ridges that separate the rows of cells are brought close together, so as to exclude these cavities from the canal through which the cud passes.

It is this beautiful and very curious mechanism which forms the peculiar character of the stomach of the camel, dromedary, and lama, fitting them to live in the sandy deserts where the supplies of water are so very precarious.

The first and second stomachs of the camel, as well as those of the bullock, are lined with a cuticle.

The third stomach of the camel is so small, and so very unlike that of other ruminants, that were it not for the distinctness of its orifices it might be overlooked. It is nearly spherical, 4 inches in diameter, is not like the third of the bullock lined with a cuticle, nor has it any septa projecting into it. The cuticle continued from the second stomach terminates immediately within its orifice, and its surface has a faint appearance of honeycombed structure; but this is so slight as to require a close inspection to ascertain it.

This cavity can answer no other purpose in the economy of the animal, than retarding the progress of the food, and making it pass by small portions into the fourth stomach, so that the process, whatever it is which the food undergoes in the third stomach of other ruminants, would appear to be wanting in the camel, and consequently not required.

The fourth stomach lies to the right of the first, and has for a great part of its length the appearance of an intestine; it then contracts partially, and the lower portion has a near resemblance in its shape to the human stomach. Its whole length is 4 feet 4 inches; when laid open, the internal membrane of the upper portion is thrown into longitudinal narrow folds, which are continued for about three feet of its length; these terminate in a welted appearance; the rugæ are large, as in the bullock, but not so prominent, nor so serpentine in

their course, and for the last nine inches the membrane has a villous appearance, as in the human stomach. Close to the pylorus there is a glandular substance of a conical form, which projects into the cavity; the blunt end of it resting upon the orifice of the pylorus. This is similar to what is met with in the bullock, but still more conspicuous.

The fourth stomach of the camel corresponds with that of the bullock in all the general characters, and resembles it in most particulars. It exceeds it in length, but the plicæ are so much smaller, that the extent of the internal surface must be very nearly the same in both. It differs from it in having a contraction in a transverse direction immediately below the termination of the plicated part, which has led both DAUBENTON and CUVIER to consider these two portions as separate cavities. I should have been induced to adopt this opinion, were it not for the circumstance of their internal structure being the same as that of the bullock, which must be admitted to be only one cavity, and as the uses of these corresponding structures must be similar, the analogy between the two is better kept up by considering it in both animals as one cavity, only remarking the contraction in that of the camel as a peculiarity belonging to ruminants without horns.

From the comparative view which has been taken of the stomachs of the bullock and camel, it appears that in the bullock there are three stomachs formed for the preparation of the food, and one for its digestion. In the camel there is one stomach fitted to answer the purposes of two of the bullock, a second employed as a reservoir for water, having nothing to do with the preparation of the food; a third so small and

simple in its structure that it is not easy to ascertain its particular office. It cannot be compared to any of the preparatory stomachs of the bullock, as all of them have a cuticular lining, which this has not; we must therefore consider it as a cavity peculiar to ruminants without horns; and a fourth, or true digesting stomach.

It is stated by authors that hares, rabbits, and even some men ruminate; their doing so is not material to the present inquiry, since their stomachs are not of that kind which makes rumination a necessary part of the process of digestion; and as far as I can learn from some persons who feed rabbits and fatten them with meal, although they have watched their rabbits with attention they never saw them bring up the food into the mouth. It may therefore be only occasional when they eat particular kinds of vegetables. They have indeed a mode of working their lips when sitting quiet, which may have been mistaken for rumination. When it takes place in men it must be considered as a disease.

From the facts which have been stated, the following gradation of ruminating stomachs is established.

The ruminants with horns, as the bullock, sheep, &c. have two preparatory stomachs for the food previous to rumination, and one for the food to be received in after rumination before it is digested.

The ruminants without horns, as the camel, dromedary, and lama, have one preparatory stomach before rumination, and, properly speaking, none in which the cud can be afterwards retained before it goes into the digesting stomach.

Those animals who eat the same kind of food with the

ruminants yet do not ruminate, as the horse and ass, have only one stomach, but a portion of it is lined with cuticle, in which situation the food is first deposited, and by remaining there some time is rendered afterwards more easily digestible when received into the other, or digesting portion.

In comparing the teeth of those animals that ruminate, with those of the horse and ass, which live on nearly the same kind of food, the following peculiarities are met with.

The ruminants with horns have molares in both jaws, and incisores only in the lower jaw.

The ruminants without horns have, in addition to these, what may be called fighting teeth, or a substitute for horns. These are tusks in both jaws, intermediate teeth between the molares and tusks, and in the upper jaw two small teeth anterior to the tusks; none of which can be of any use in eating.

The camelo-pardalis forms an intermediate link in these respects. It has short horns, and has no tusks.

The molares in both these genera of ruminants are open in the structure of their crown, which is not horizontal but oblique, the outer edge in the upper jaw and the inner in the lower jaw being the most prominent, so as to adapt them to each other. The lower jaw has less width than the upper, so that the lower molares fall considerably within the upper; when the animal eats it can only masticate with one side of the mouth at a time, by bringing the lower jaw to that side, so as to make the teeth of both jaws oppose each other; the teeth of that side are applied to the food three or four times, and then those of the opposite side.

This mode of mastication appears to be peculiar to the ruminants, and is certainly very different, and much more imperfect than the mastication of the horse, whose molares are very compact in the texture of their crowns, and are opposed directly to each other by horizontal planes.

Letter from Charles Hatchett, *Esq. concerning some Peculiarities in the Urine of the Camel.*

DEAR SIR, April 30, 1806.

Being a short time absent from my house, and not having at hand any apparatus to examine the camel's urine, which you lately sent to me, I delivered it to my friend, Mr. W. BRANDE, of Arlington-street, who has on several occasions much distinguished himself in chemical science, and I now have the pleasure of transmitting an account of the results of his comparative experiments on the urine of the camel and the cow, which, I think, appear to be highly deserving of attention.

The presence of uric acid in the former, and that of phosphat of lime in both, are new facts, which reflect additional light on the composition of the urine of graminivorous animals.

Mr. BRANDE first states his experiments on the camel's urine as follows:

" I divided it into two equal portions, taking half for distillation, which was performed at a very low temperature.

" When somewhat more than three-fourths had passed over, the residuum in the retort became thick, assuming a deep brown colour, and having a peculiar fetid odour. I now stopped the distillation, and affused alcohol, with a view of

ascertaining whether it contained urea. This I obtained in a considerable proportion. It had the same appearance and properties as that which is afforded by human urine. What remained after the separation of the urea, consisted chiefly, as far as I could ascertain, of muriat of potash, with a little muriat of ammonia, phosphat of lime, and probably urat of potash.

"I may here remark that no benzoic acid was separated towards the latter part of the distillation, nor could I obtain any from the residuum.

"The remaining portion of the urine was examined by the following tests:

"Nitrat of silver occasioned a very copious precipitate, which became speedily black on exposure to light.

"Muriat of barytes indicated the presence of a minute portion of sulphuric acid.

"Ammonia threw down a little phosphat of lime. When muriatic acid was poured into the urine, an effervescence was produced by the emission of carbonic acid gas.

"A portion of the urine, which had been exposed to the air for some days, deposited a sediment, which when treated with nitric acid, and evaporated, assumed a red colour, and thereby shewed the presence of uric acid.

"From the results of these experiments, and of some others, which I do not think it necessary to mention, I have drawn the following conclusions relative to the component parts of camel's urine; but as the quantity, upon which I operated was small, they must only be regarded as an approximation to the truth.

Water - - - - -		75
Phosphat of lime - -	}	
Muriat of ammonia - -	}	
Sulphat of potash - - -	} -	6
Urat of potash - - -	}	
Carbonat of potash - -	}	
Muriat of potash - - -		8
Urea - - - - - -		6
		95."

Mr. BRANDE next proceeds to give an account of his examination of cow's urine.

" As I had sent me a large supply of cow's urine, I have been enabled to vary my experiments on it, in such a manner, that I hope to have drawn tolerably accurate conclusions with respect to its composition.

" The analysis was conducted nearly as follows:

" 1. I put four ounces into a glass retort, to which a proper apparatus was adapted for collecting its gaseous as well as fluid parts. The distillation was performed in a sand-bath.

" I obtained carbonic acid and water, shewing some signs of ammonia; possessing however a peculiar flavour. There remained in the retort a brown mass, which was chiefly composed of muriat of potash, and of ammonia; sulphat of potash, phosphat of lime, and urea.

" The carbonic acid must in part have been produced from a decomposition of a portion of urea: and hence the brown colour of the residuum.

" 2. Four ounces of the urine were evaporated to half the quantity. Muriatic acid was added, and a precipitate was

formed, from which I obtained a small portion of benzoic acid.

" It is somewhat remarkable that no traces of this substance should have been discovered in the residuum left after distillation; nor could I by any means observe its presence before heat had been employed.

" I mention this circumstance, as I think it coincides with your opinion respecting the formation of this acid; and that in this case it is not an educt, but a product.

" Still I do not see why by a similar process I could obtain none from the urine of the camel.

" 3. The cow's urine was then examined by the following reagents.

" Nitrate of silver caused an abundant precipitation of muriate of silver.

" Barytes was thrown down in the form of sulphat and carbonat: the latter in the smallest proportion.

" Ammonia indicated the presence of phosphat of lime.

" Carbonic acid gas was extricated by muriatic acid.

" The results, which I had obtained from the analysis of camel's urine, induced me to imagine that uric acid might possibly exist in the urine of other graminivorous animals; and indeed it was a natural conclusion: but I find that it is not the case, for in the present experiments I have been unable to detect even the least trace of that substance.

" The following estimation of the relative proportions of the substances present is, I think, as correct as the nature of the subject will allow.

"100 parts contain

Water - - - -	65
Phosphat of lime - - -	3
Muriat of lime - - - } -	15
——— ammonia - - }	
Sulphat of potash - - -	6
Carbonat of potash - - - } -	4
——— ammonia - - }	
Urea - - - -	4
	97.

"The loss may be attributed to animal matter, probably albumen and gelatine.

"I have for obvious reasons omitted the benzoic acid.

"The principal and only essential difference between the urine of the camel, and that of the cow, is that the former contains uric acid. They both appear to be destitute of soda.

"It will also appear that in the present instance the latter contains a larger proportion of saline matter, but this can only be regarded as a casualty, when we consider the variation, to which the relative proportion of water to the salts of urine is liable, according to the circumstances under which the secretion takes place."

From Mr. BRANDE's experiments on the urine of the camel, it appears that (exclusive of water) the principal ingredients are muriat of potash, and urea; and as ammonia is present only in a very small proportion, which is even less than in the urine of the cow, we may conclude that the various accounts, which state the urine of the camel to have much contributed to the

production of muriat of ammonia, or sal ammoniac, are without foundation.

It is remarkable that uric acid should be found in the camel's urine, and I believe it is the first instance on record, as far as relates to the urine of graminivorous animals.

Mr. BRANDE's experiments also show that phosphat of lime is present in the urine of these animals, which is in opposition to the hitherto received opinion.

FOURCROY and Dr. THOMSON have quoted the analyses of camel's and cow's urine made by ROUELLE, and it may not be improper to compare them with those of Mr. BRANDE.

Component Parts of Camel's Urine.

BRANDE.		ROUELLE.*
Water - - -	75	
Phosphat of lime -		
Muriat of ammonia -		Carbonat of potash
Sulphat of Potash -		Sulphat of potash
Urat of potash -		
Carbonat of potash -		Muriat of potash
Urea - - -		Urea.
Muriat of potash -	8	
	95	

* THOMSON's System of Chemistry, 2d edit. Vol. IV. p. 655.

Component Parts of Cow's Urine.

BRANDE.		ROUELLE.
Water - - -	65	
Phosphat of lime -	3	Carbonat of potash
Muriat of potash -	} 15	Sulphat of potash
——— ammonia -		
Sulphat of potash -	6	Muriat of potash
Carbonat of potash -		
——— ammonia -		Urea
Urea - - -	4	Benzoic acid.
	97	

From this comparison, we see that Mr. BRANDE and M. ROUELLE agree in stating that potash is the only fixed alkali present in both cases.

The urine of the rabbit, according to VAUQUELIN, is composed as follows.

Carbonat of lime
 · magnesia
 · potash
Sulphat of potash
——— lime
Muriat of potash
Urea
Gelatine
Sulphur.

The same chemist found that the urine of the guinea-pig deposited carbonat of lime: that it changed the colour of syrup of violets to green; and that it contained carbonat, and muriat of potash; but not any phosphat, nor uric acid.

From all these it therefore appears, that soda and its combinations do not form any part of the urine of the camel, cow, guinea-pig, and rabbit; unless we may be permitted to believe that the composition of the urine of animals in general, is various at different periods, not only in the proportions of the ingredients, but also in the quality.

Should however the contrary of this be the case, we may assert that the urine of the horse is peculiarly distinguished from that of the above mentioned animals, by the presence, and abundance of soda, as the following analysis made by FOURCROY and VAUQUELIN will demonstrate.

*Component Parts of the Urine of the Horse.**

Carbonat of lime - - -	0011
——— soda - - -	0009
Benzoat of soda - - -	0024
Muriat of potash - - -	0009
Urea - - - -	0007
Water and mucilage - - -	0940
	1000.

Now unless the urine of animals is very differently composed at different periods, there is in this instance a marked chemical character between the urine of the horse, and that of the above-mentioned graminivorous animals: and if so, perhaps the same may prevail respecting those other animals, which are the most nearly allied to the horse.

This certainly merits investigation; but it can only be accomplished by a number of comparative experiments, and

* THOMSON, Vol. IV. p. 655.

analyses made on the urine of the same and of different animals at various times, and under different circumstances.

I am, &c.

CHARLES HATCHETT.

P. S. Since the foregoing letter was written, Mr. W. BRANDE has examined the urine of the horse and ass; the result is as follows:

"The urine of the horse is turbid, and of a mucilaginous consistence; it changes the colour of vegetable blues to green, and when exposed to the air it becomes covered with a thin pellicle of carbonat of lime.

"When evaporated to the consistence of thick honey, it yields to alcohol a small portion of urea. The salts which it contains are as follows:

Carbonat of lime
———— soda
Sulphat of soda
Muriat of soda
Benzoat of soda
Phosphat of lime.

"These amounted in the present instance to about one-eighth of the urine. I could discover no trace either of potash or ammonia.

The urine of the ass is somewhat mucilaginous: but at the same time transparent. Like that of the horse it changes vegetable blues to green; but it deposits no carbonat of lime.

"It differs in its composition from that of the horse, by containing a much greater relative proportion of phosphat of

lime and urea: it contains also carbonat, sulphat, and muriat of soda, and there appears to be a small quantity of potash, which is probably united to muriatic acid. I could not discover any benzoic acid.

"It is worthy of remark that the urine of the horse and ass are both destitute of ammonia."

Explanation of the Plates.

Plate XV.

Represents a longitudinal section of the first stomach of the bullock, showing its cavity, which is made up of two compartments, separated from each other by two strong transverse ridges, composed of a mixture of ligamentous and muscular fibres: also the opening into the second stomach, and a portion of its cavity: the orifice leading into the third stomach, and the canal through which the food is thrown up from the second stomach into the mouth, and afterwards conveyed into the third stomach.

a, The œsophagus terminating in the first stomach.

bbbb, The cavity of the first stomach.

cc, The two ridges dividing it into two compartments.

dd, The mouth of the second stomach.

e, The orifice leading to the third stomach.

ff, Two muscular bands, which have their origin from the coats of the first stomach and terminate in the orifice of the third, forming a canal, along which the food is conveyed from the second stomach, to the mouth, and from the mouth to the third stomach.

Plate XVI.

Represents a posterior view of the first and second stomachs of the bullock unopened, and an internal view of the third and fourth stomachs, in their natural relative situation to the others.

a, The œsophagus.

bb, The coats of the first stomach, in a distended state.

c, The coats of the second stomach.

d, The orifice leading into the third stomach.

eee, The plicæ of three different breadths, which are contained in the third stomach.

f, The valvular termination of the third stomach in the fourth.

ggg, The longitudinal plicæ of the fourth stomach.

h, The rugæ of the fourth stomach, near the pylorus.

i, The glandular projection opposed to the orifice of the pylorus.

k, The pylorus, or termination of the fourth stomach.

Plate XVII.

Represents an internal view of the first stomach of the camel, exposed in the same manner as that of the bullock, in Pate XV.

In this stomach there are two compartments, separated from each other by a longitudinal ridge, which is composed of strong muscular fibres, and the orifice leading into the second stomach is nearly at right angles to the cavity of the first; there is a strong muscle passing from the orifice of the first stomach through the upper part of the second stomach to the third, where it terminates; this muscle, and the

longitudinal ridge form a canal along which the ruminated food passes into the third stomach.

a, The œsophagus.

bb, The longitudinal ridge, dividing the cavity into two compartments.

cc, The muscle which passes to the third stomach.

d, The opening into the second stomach.

ee, The muscular cells on the right side of the cavity.

ff, The larger cells on the left side, which serve to moisten the food lying over them, and make it of a fit consistence to be regurgitated into the mouth along the canal formed by the longitudinal ridge and the muscle going to the third stomach.

gg. A broad muscular band separating the cellular structure into two portions.

Plate XVIII.

Represents a posterior view of the first stomach of the camel unopened, and an internal view of the second, third, and fourth stomachs, in their relative situation to the first, similar to the view given of the stomachs of the bullock, in Plate XVI.

a, The œsophagus.

bb, The coats of the first stomach, in a distended state.

c, The communication between the first and second stomachs.

dd, The muscle running along its upper part to terminate in the orifice of the third stomach. This muscle when it acts with its greatest force brings forward the orifice of the third stomach nearly close to that of the second, and by so doing shuts up the rows of cells in the lower part of the cavity so that no part of the solid food can pass into them.

ee, The rows of cells which form a reservoir for the water.

f, The opening leading into the third stomach.

g, The cavity of the third stomach.

h, The orifice of the fourth stomach.

ii, The longitudinal plicæ of the fourth stomach.

kk, The rugous structure of the lower part of the fourth stomach.

l, The glandular projection opposed to the orifice of the pylorus.

m, The pylorus.

n, A dilatation or membranous cavity between the pylorus and duodenum.

o, The duodenum.

Plate XIX.

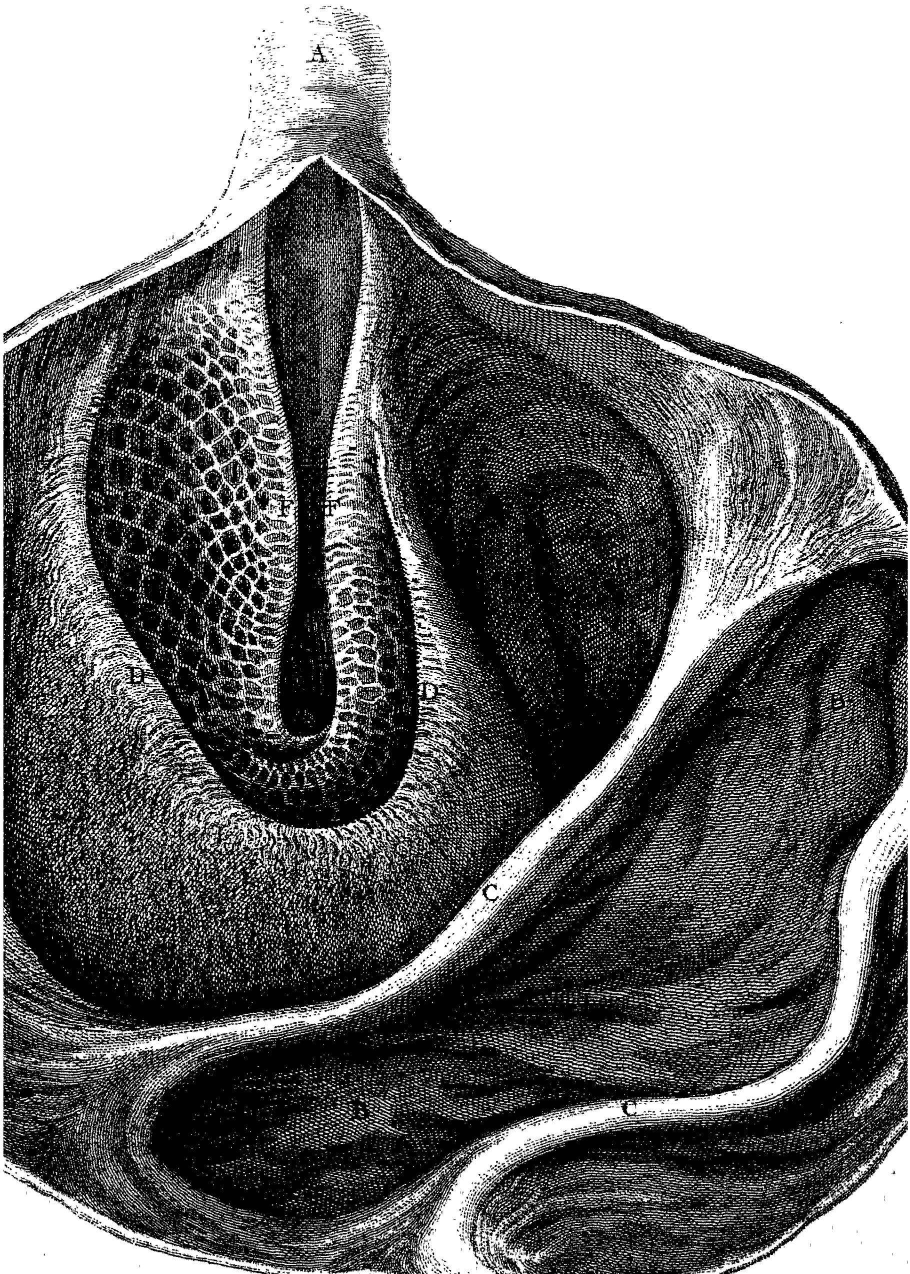
This plate is intended to show the directions of the muscular fibres which run upon the orifices and sides of the cells in the first and second stomachs of the camel.

It represents six of the cells in the lower part of the left side of the first stomach, with a portion of the longitudinal ridge by which they are bounded. These particular cells were chosen in preference to those of the second stomach, as they were the largest, and the muscular fibres were most distinctly seen; the same structure is met with in the cells of the second stomach.

aa, The longitudinal ridge, to show its muscular structure, and the mode in which the fibres go off to furnish the orifices of the cells.

bbbbb, The course of the fibres going from cell to cell to their orifices.

The [illegible]scular fibres by which the cells are enabled to throw ou[illegible] contents.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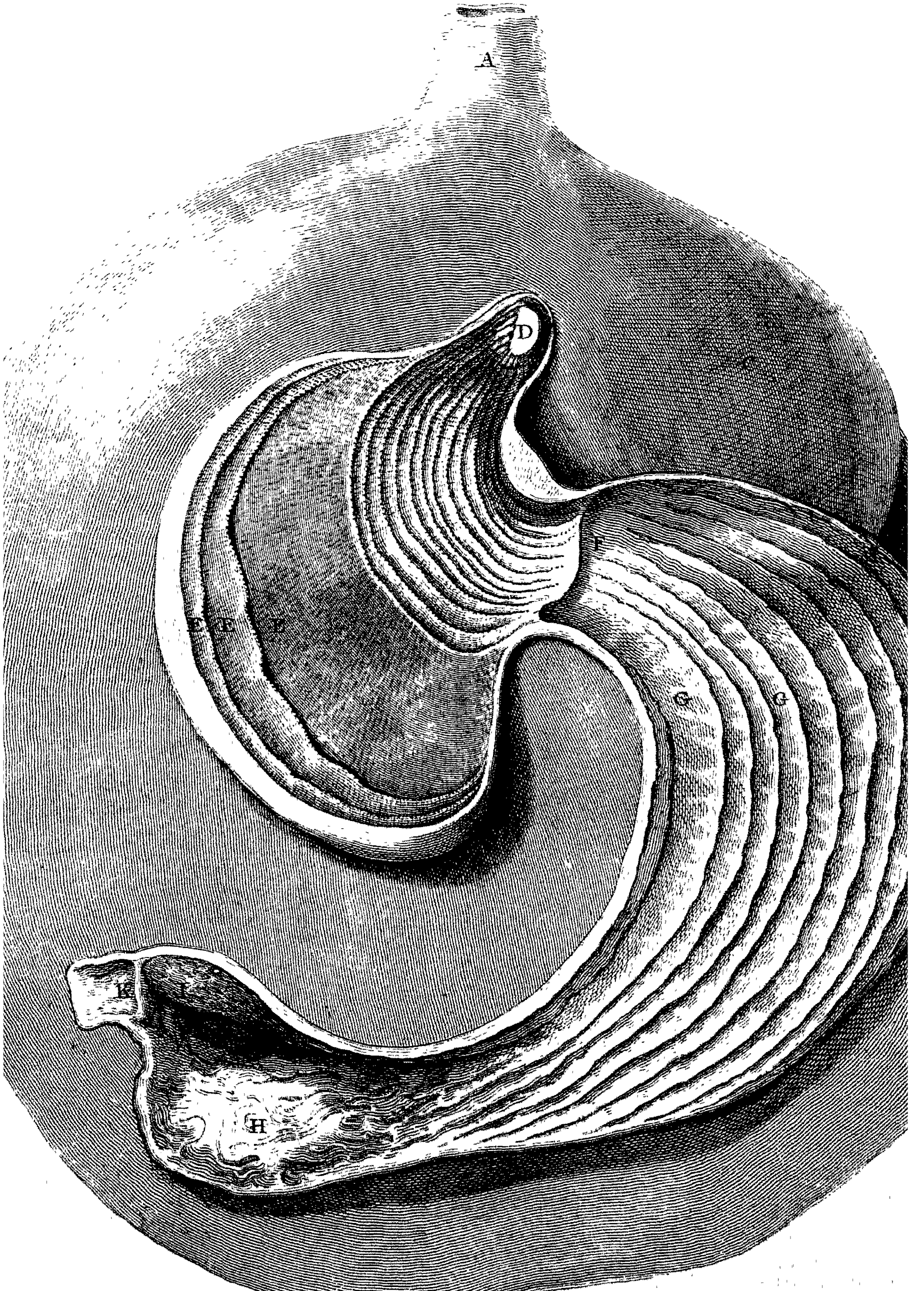
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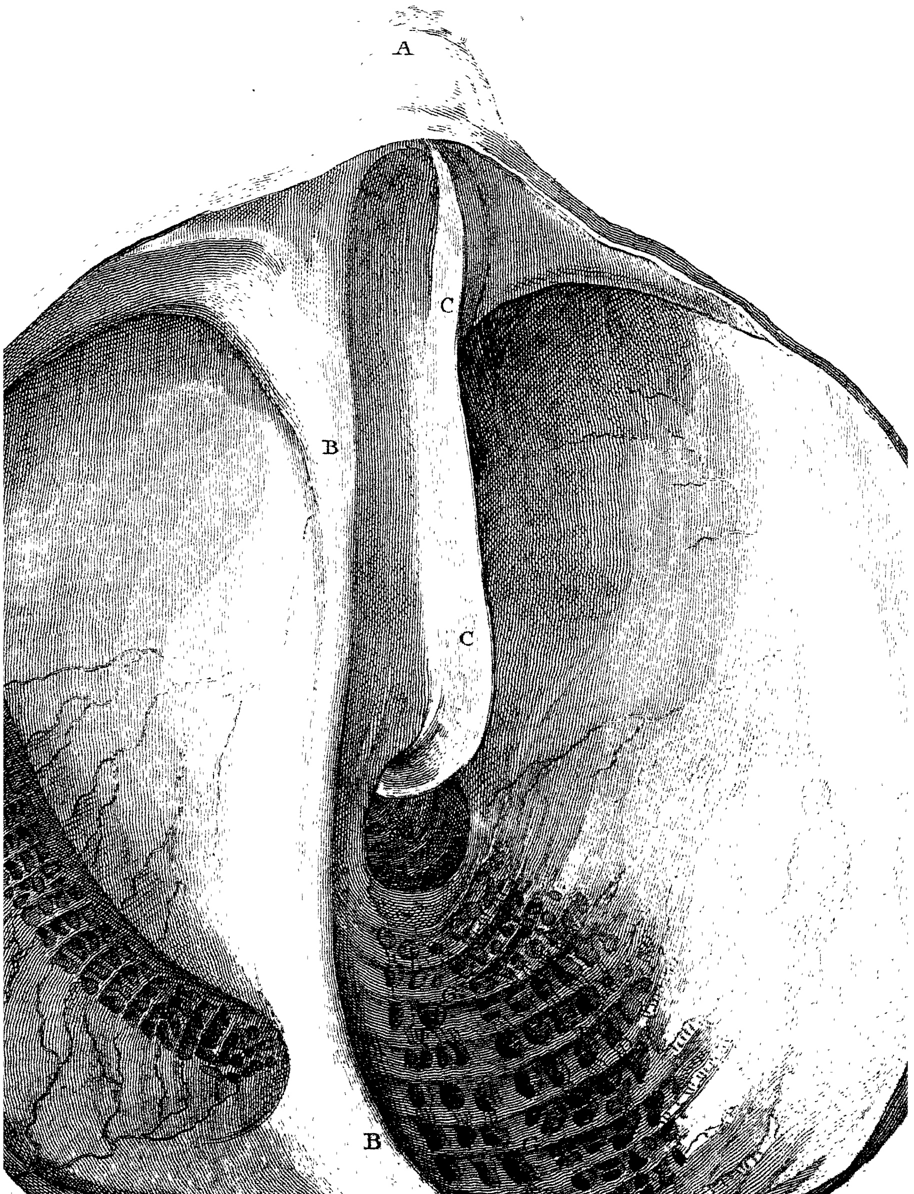
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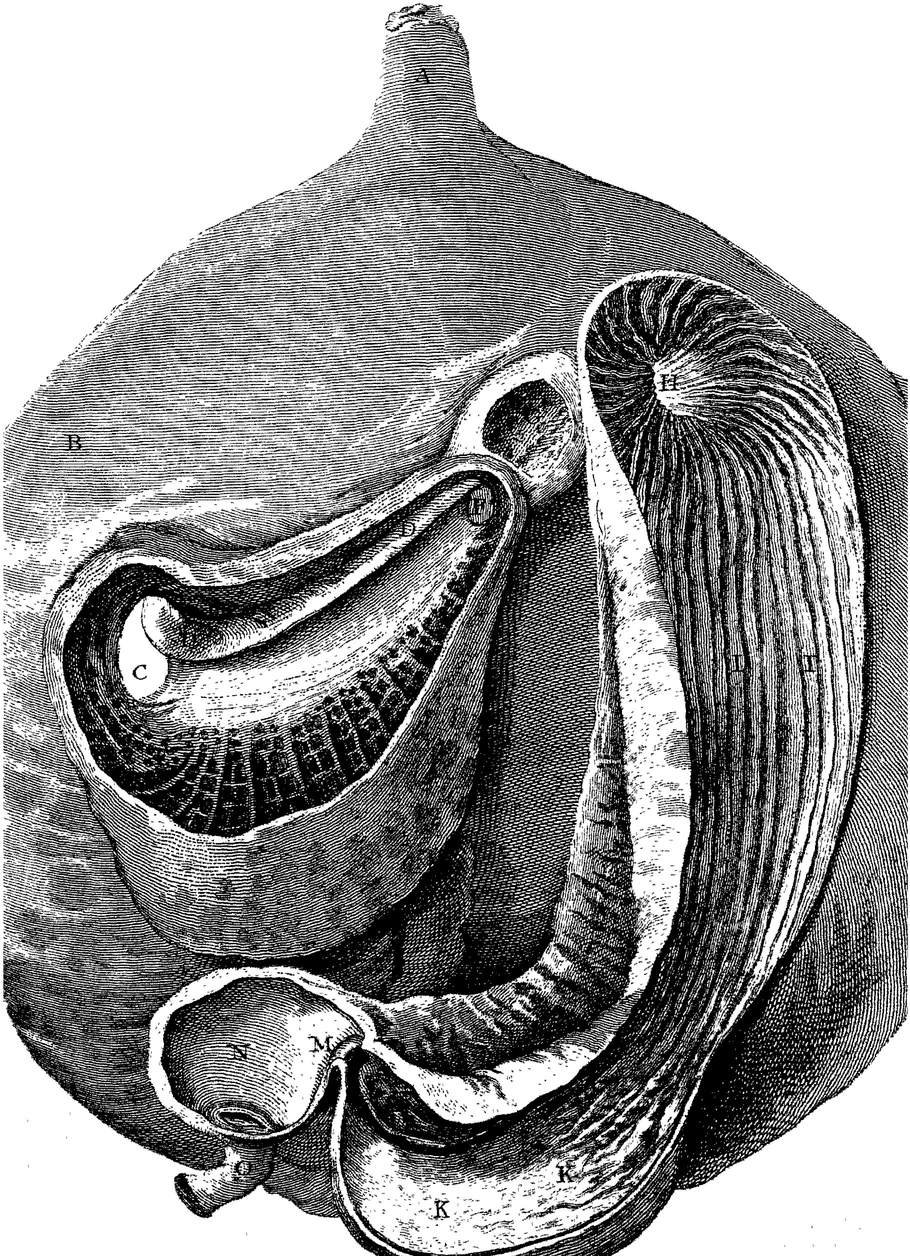
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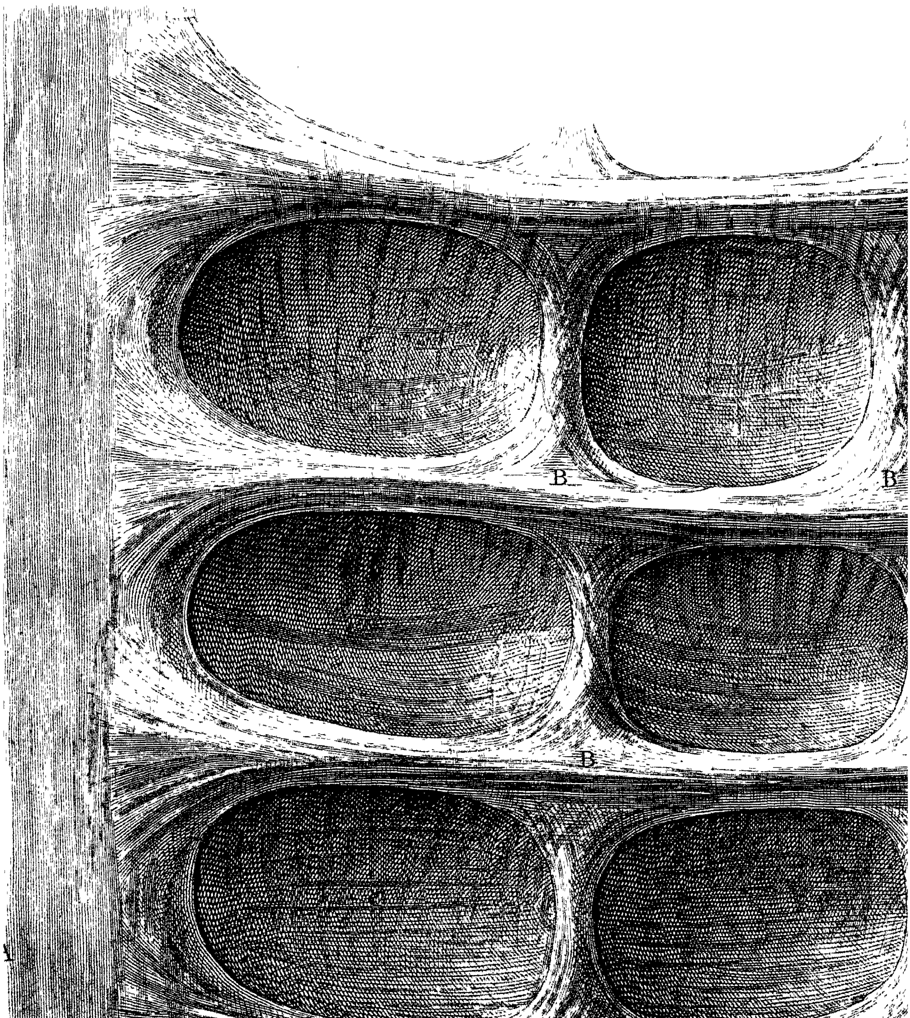
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XX. *Observations on the Variation, and on the Dip of the magnetic Needle, made at the Apartments of the Royal Society, between the Years* 1786 *and* 1805 *inclusive. By Mr.* George Gilpin. *Communicated by* Henry Cavendish, *Esq. F.R.S.*

Read June 19, 1806.

Of the Variation Compass.

THE variation compass used in making the following observations is the same instrument used in former observations of the variation, and published by the Society in several volumes of their Transactions: and as a particular and accurate description of its construction was given by HENRY CAVENDISH, Esq. F.R.S. in the LXVIth volume, it will not be necessary to say any thing here on that subject. But these observations being the first that have been communicated since the compass was put up in the Society's apartments in Somerset Place, it may not be amiss to point out its situation in the house at the time of observation, and the method pursued to attain such allowances as were proper to be made in deducing the results here given.

1. The compass in the house, at the time of observation, was placed in the middle window, on the south side of the Society's meeting-room, upon a strong mahogany board $1\frac{1}{2}$ inch thick. Against the opposite building the dial-plate of a watch is fixed, making an angle with the true meridian of $31° 8',8$ to the eastward, as a mark to which the telescope of

the compass was adjusted. To obtain the angle that this mark made with the true meridian, I fixed a transit-instrument on the mahogany board above mentioned, precisely in the same place where the compass had been placed, and having adjusted its telescope to the said mark, the transits of the sun and stars over a vertical circle passing through the zenith and this mark, were observed; and the angle contained between the said mark and the true meridian, was found by computation to be 31° 8',8 as above.

2. For the purpose of ascertaining what error there might be, from a want of parallelism between the line joining the indices and the magnetism of the needle, and thereby to determine whether in the usual method of observing, the indices shew the true angle which the direction of magnetism makes with the first division or zero, a great many observations were made on both ends of the needle, and with both sides of the needle uppermost, (the cap of the needle being made to fit on readily on either face for this purpose,) *viz.* north end and south end in its upright position, and north end and south end with the needle inverted, and the mean of the four giving the angle greater by 2', than that shewn by the north end in the upright position of the needle, (which was the end always used in these observations,) two minutes have been added to all the observations read from the instrument, as the correction for this error to angles on the east side of zero, and subtracted from angles on the west side, to obtain the true angle; which error to angles on the west side, however, only occurred, when the instrument was taken out of doors to determine the effect of the iron work of the building.

3. The variation compass being placed in the house for observation, could not be supposed to be entirely out of the influence of iron; I was therefore desirous to ascertain how far that influence might extend; for the determination of which, the following method was adopted.

Having caused to be sunk into the earth to some depth a strong post, in the wood yard of Somerset House, at a considerable distance from the influence of any iron, on which the compass might be placed, and from which station, there was a convenient mark at a proper distance to which its telescope could be adjusted; I took the compass there at those times of the day when the needle was stationary, *viz.* morning and afternoon: before the compass was carried out of doors, observations were made in the room; then it was taken out of doors to the above mentioned station for observation there; and the observations were again repeated after the compass had been restored to its situation in the room; so that had any alteration taken place in the interval, such alteration would have been detected; but during the whole series, no material difference occurred between the observations made in the house before, and after those taken in the yard.

The observations therefore made in the yard, compared with those taken in the house, both before and after those taken out of it, formed the comparison for obtaining the error, or the effect of the iron work of the room on the needle in the house, and there is reason to believe that considerable accuracy has been obtained. They are as follow.

By a mean of 20 sets, or 200 observations taken with the compass in the yard, compared with twice that number taken in the house, before and after those taken in the yard, the

variation observed in the house was found to be greater than that observed in the yard by 5',4. The mean of nine sets of observations taken in the morning giving for the error 5',5. And the mean of eleven taken in the afternoon giving for the error 5',3. The variation in those tables have therefore been lessened by the above mentioned quantity 5',4, as the error for the effect of the iron work of the room on the needle in the house.

I must not omit to mention that of these 20 sets of observations mentioned above, nine only were made with the compass in the same situation, and eleven in that of a different one; for, after nine sets had been taken, a pile of boards was put up between the compass and the mark to which it had been adjusted, which made it necessary to remove the post on which the compass had been placed, a few feet to the westward of its former situation, to clear it from the said pile of boards; and eleven sets of observations were made from this new station, with the compass adjusted to the same mark it had been adjusted to before, and the angles that this mark made with the true meridian from each of these stations, were ascertained by placing a transit-instrument precisely where the compass had been placed, and observing transits of the sun and stars, in the same manner as has been described in finding the angle of the mark that the compass was adjusted to in the house. And it is conceived that this accidental circumstance adds some weight to the accuracy with which these operations were performed, as the error from the two results of nine, and eleven, does not differ so much as 0'5 from each other.

Dipping Needle.

The dipping needle with which the observations in this communication were made, being the same instrument used in former observations of the dip, and it having also been described by Mr. Cavendish in the Paper before alluded to, it will not be necessary to say any thing of its construction here. Its situation in the house was in the eastern window in the meeting-room, next the door.

As the observations made with the dipping needle were not affected by any other source of error than that of the iron work of the room, in order to ascertain the quantity of error, the instrument was taken out of doors at two different times, after an interval of ten years, differently situated each time, and the observations made at both these times out of doors, compared with the observations made in the room, giving for the error 20′ more than the dip was found to be in the room, and both agreeing to one minute; that quantity has been added to all the observations made with the dipping needle in the room for its error, as affected by the iron work of the room.

Although a valuable Paper on the diurnal variation of the horizontal magnetic needle, by the late Mr. JOHN CANTON, F. R. S. was published in the first part of the LI[st] volume of the Phil. Trans. for the year 1759, containing a great number of observations made at different and irregular times of the day throughout the year; yet, it appeared to me, that if the variation were to be observed at short but stated intervals of the day for one year, the results would perhaps not only prove more satisfactory in determining the times of the needle becoming stationary, but would show its progressive and regressive motions better, than if observed at irregular intervals. To effect which, I imposed this laborious task upon myself for the space of sixteen months.

The observations contained in Table I. in sixteen pages, *viz.* from September, 1786, to December, 1787, both inclusive, are the results, made at many but stated times of the day, and so disposed, that the progress, or regress, of the variation, may be readily seen by mere inspection.

Table II. contains the mean monthly variation for the above mentioned times of the day contained in Table I.

Table III. contains, besides the mean monthly true variation, and mean monthly diurnal alteration of variation, for the sixteen above mentioned months, the mean monthly true variation, and diurnal alteration of variation for many months in the year, between the years 1786 and 1805 inclusive.

The numbers put down in Table I. are each of them a mean of five observations, and often more.

Those in Table II. depend on Table I.

As the observations from which the true variation has been given in Table III. between the years 1788 and 1805 were too numerous to be all inserted, it has been thought sufficient to

give the mean monthly true variation, and mean monthly diurnal alteration of variation only; and they were determined from a mean of the observations made at those times of the day when the variation was considered least, and greatest; which variations for each month, may generally be considered as a mean of 600 observations.

From the observations made by the late Dr. HEBERDEN and others, about the year 1775, the variation was found to increase annually nearly 10′, since that time to the present, its rate of increase has been considered as gradually diminishing,* and for the last three or four years the alteration has been so very small as to make it somewhat doubtful whether it may

* An exception to the progressive increase appears between the years 1790 and 1791, as the observations between these two years make it to decrease 2 or 3′, and subsequent observations to increase again; to what this should be attributed, I am at a loss to account, unless it arose from the alteration which took place in the iron work of the room in December, 1790; four strong iron braces having been applied to the girders in the floor of the great room of the Royal Academy, (which is over the Society's meeting-room,) in consequence of a cracking noise made from the great pressure of a number of persons in the room during the time that Sir JOSHUA REYNOLDS was delivering a lecture; these braces were applied two on each side of, and equidistant from, the compass, the nearest, about 18 feet from it. It may be proper to mention, however, that having been favoured with the variation observed both by Mr. CAVENDISH and Dr. HEBERDEN, in the above mentioned years the alteration of the variation was by the former nearly the same as in my own, but by those of the latter greater in both cases.

An alteration took place between the observations made with the dipping needle in the same years. All the iron braces were on the north west side of the needle, and the nearest about 18 feet from it.

The allowances made to the observations of the variation, and also of the dip, for the effect of the iron work of the room, were both ascertained after the above mentioned alteration in the iron work took place, but they have, notwithstanding, been applied to the observations made before as well as since that time.

not be considered stationary, but I would not from so short a period conclude that it really is so.

From the observations of sixteen months, *viz.* from September, 1786, to December, 1787, both inclusive, the variation may be considered as generally stationary at or about 7 or 8 o'clock in the morning when it is least; and about 1 or 2 o'clock in the afternoon when it is greatest; and therefore it has been the practice in determining the true variation put down in these tables, to take a mean of the two morning, and the two afternoon observations, made at those times, for the true variation.

In March, 1787. The mean monthly diurnal alteration of variation was found to be 15',0; in June 19',6; in July 19',6; in September 14',8; and in December 7',6. But on a mean of 12 years observations, from the year 1793 to 1805, the diurnal alteration of variation in March was only 8',5; in June 11',2; in July 10',6; in September 8',7; and in December 3',7.

Table IV. contains the differences for 12 years, *viz.* from 1793 to 1805, between the observations of the variation made in the months of March, June, September, and December, or at the times of the vernal and autumnal equinoxes, and summer and winter solstices; by a mean of these 12 years, the variation appears to increase or go westward, from the winter solstice to the vernal equinox 0',80; diminishes or goes eastward from the vernal equinox, to the summer solstice 1',43; increases again from the summer solstice to the autumnal equinox 2',43; and continues nearly the same, only decreasing 0',14, from the said equinox to the winter solstice.

These differences at the times of the equinoxes and sol-

stices have been noticed by M. CASSINI, in his observations made at the Royal Observatory at Paris, between the years 1783 and 1788, but the effect was considerably greater in his observations, than in those mentioned above; his results however were, in my opinion, drawn from too few observations, being from only 8 days observations about the times of the equinoxes and solstices, which differ considerably among themselves; and experience teaches us, that magnetical observations made for a period so limited are not sufficient for minute purposes: I have therefore, in the results here given, taken the mean of the observations made during the whole month in which the equinoxes and solstices fall, which appear to me likely to furnish results more satisfactory; and all the foregoing observations are to be considered as the results or mean of a great many, by way of arriving at greater accuracy than could be obtained without; this, however, was found to be more necessary at some times than at others; sometimes, the needle would be extremely consistent with itself, so as to return exactly to the same point, however often it might have been drawn aside; at other times it varied 2 or 3′, sometimes 8, 10′, or even more; this uncertainty in the needle arises principally, I believe, from changes in the atmosphere, for, a change of wind, from any quarter to another, almost always produced a change in the needle from steady to unsteady, and *vice versâ*, but it was generally more unsteady with an easterly wind, than when it blew from any other quarter, and most steady when the wind was south or south-westerly. An Aurora Borealis always produced considerable agitation of the needle.

It has been mentioned in this Paper, that the annual increase

of variation was found about the year 1775 to be nearly 10′; and was considered at that time to be gradually diminishing, but it is remarkable that this rate of increase appears from the annexed Table to be nearly the same at which it has been found to move between all the different periods in the said Table, from 1580 to 1787, a period of more than 200 years, excepting between the years 1692 and 1723; the observations of Dr. HALLEY in 1692 and Mr. GRAHAM in 1723 make the annual increase 16′; to what this difference could be owing I am at a loss to account; on referring to observations made at Paris for those two years the annual increase is 14′; subsequent observations made by Mr. GRAHAM in 1748 make the annual increase between this year and 1723 only 8′,1 nearly what its rate had been found before this great difference occurred; and from the variation of Mr. GRAHAM in 1748, and the variation observed by Dr. HEBERDEN in 1773, the annual increase is 8′,4; the variation in 1773 compared with the variation observed by myself in 1787, give for the annual rate of increase 9′,3; but between 1787 and 1795, the annual increase was only 4′,7; between 1795 and 1802, 1′,2; and between 1802 and 1805, only 0′,7.

The mean rate of annual increase for the above mentioned period of 207 years, *viz.* from 1580 to 1787, is 10′.

As there appears something curious in the rate at which the variation has been moving from observations made at London, for a period of more than 200 years, the annual increase of which during that time continued nearly the same, but in a subsequent period of 18 years only, the decrease of that annual increase became so rapid, that the annual increase in the latter part of it does not amount to quite one minute,

I shall subjoin the following Table, by way of elucidating what is here mentioned.

By whom the Variation was observed.	Year.	Variation.	Annual Increase.
		° ′	′
Mr. BURROWS,* in - -	1580	11 15 E	
Mr. GUNTER - - -	1622	6 0	+ 7,5
Mr. GELLIBRAND - -	1634	4 6	9,6
Mr. BOND † - - -	1657	0 0	10,6
Mr. GELLIBRAND ‡ -	1665	1 22 W	10,2
Dr. HALLEY § - -	1672	2 30	9,7
——— - -	1692	6 0	10,5
Mr. GRAHAM ‖ - - -	1723	14 17	16,0
——— - -	1748	17 40	8,1
Dr. HEBERDEN ¶ - -	1773	21 9	8,4
Mr. GILPIN - - -	1787	23 19	9,3
——— - -	1795	23 57	4,7
——— - - -	1802	24 6	1,2
——— - -	1805	24 8	0,7

* The observations of BURROWS, GUNTER, and GELLIBRAND's, in 1634, are taken from SELLER's Practical Navigation, 1676. BURROW's observations are said to be the oldest and best in the world; longitude and latitude found by dipping needle, p. xvi. GELLIBRAND is said to be the first person who ascertained the variation of the variation, about the year 1625, Phil. Trans. No. 276—278; but if this is the date of the observations by which it was determined, the observations of GUNTER in 1622, show him to have a prior claim; BOND, in his Longitude found, p. 5 and 6, says that the variation was first found to decrease by Mr. JOHN MAIR, secondly by Mr. EDMUND GUNTER, thirdly by Mr. HENRY GELLIBRAND, and by himself in 1640.

† Longitude found, p. 3.

‡ Ibid. p. 13; and Longitude and Latitude found by Dipping Needle, p. 6.

§ Phil. Trans. No. 195, p. 565.

‖ Ibid. No. 383, p. 107; and No. 488, p. 279.

¶ Obligingly communicated by his son, the present Dr. HEBERDEN.

Table V. contains the dip of the magnetic needle from the years 1786 to 1805. For the first sixteen months, *viz.* from September, 1786, to December, 1787, both inclusive, the dip was observed as frequently as the variation, *but as there does not appear to be any diurnal alteration in the dip*, to make it at all interesting to communicate so many observations as were made, the mean therefore for each month has been thought sufficient for insertion.

To explain the foregoing Table it must be observed, that each of the numbers in the four first columns of the above Table, are each of them the mean of several means, as expressed in the line against those numbers; and as each of those means, are again the mean of five observations at least, each of the numbers in the first line, said to be the mean of nine means, is therefore a mean of forty-five observations; and so of all the rest.

The numbers in the fifth column, entitled true dip, are the means of the numbers contained in the four preceding columns in the same line with it.

The dipping needles used by NORMAN, the inventor of the dipping deedle, who observed the dip at London in the year 1576 to be* 71° 50′; and of Mr. BOND, who observed it in 1676 to be† 73° 47′; not being so much to be depended upon as the needles that have been in use for near a century past, render the progressive increase of the dip from NORMAN'S time, to the time of its maximum, somewhat doubtful. But Mr. WHISTON, whose needle there is reason to believe was more to be relied upon, in the year 1720 determined the dip

* New Attractive, c. 4.

† Longitude found.

to be* 75° 10′; this, when compared with many, and very accurate observations made by Mr. CAVENDISH, with several needles in the year† 1775, who found it to be 72° 30′, makes the decrease in this period of 55 years on a mean, 2′,9 *per annum*. And from a comparison of my own observations of the dip in 1805, which was 70° 21′, with the above of Mr. CAVENDISH in 1775, its annual decrease, on a mean, appears to have been 4′,3; and its progressive annual decrease on a mean in the above mentioned period of 30 years 1′,4.

I cannot conclude this Paper without expressing my regret, that so little avail should have been made of the numerous opportunities which have been afforded to travellers and others in the last century for making accurate observations with proper instruments, at land, on the variation in different parts of the world: such observations would probably have afforded some curious and useful facts which would have materially assisted in forming a theory much more certain than what we at present possess; the present received opinion of the cause of the diurnal alteration of variation would be confirmed or invalidated; its quantity of effect in different places, a most desirable acquisition, would be ascertained; and we should be put in possession of more valuable and correct information on the variation than can be derived from observations made with the common azimuth compass, even at land, owing to its imperfect construction. The variation thus accurately obtained at any one period, compared with the variation correctly ascertained at a subsequent period, would

* Longitude and Latitude found by Dipping Needle, p. 7—94.

† Phil. Trans. Vol. LXVI. p. 400.

give a rate of alteration of the variation which could be relied on.

The celebrated HALLEY thought the variation of so much importance, that he made two voyages for the purpose of making observations on the variation, to confirm his theory advanced in 1683, and soon after he published his variation chart. Since his time no better theory than he left has been obtained, although it must be confessed that many observations have been made at sea by voyagers; but these observations made generally to answer the purpose of the observer at the time only, are therefore seldom preserved; for unless made by authority, which rarely happens, they do not often meet the public eye; and it must be from observations made with care, and with good instruments, carefully registered, and properly arranged, that any real advantage can be derived. It is hoped therefore, that in future attention to this subject will not be thought beneath those who may have it in their power essentially to promote an undertaking so interesting to the philosopher, and so valuable and useful to the maritime world.

Table I.

Observations on the Variation of the magnetic Needle.

1786	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M
Sep. 1	23° 7'	23° 10'	23° 18'	23° 27'	23° 29'	23° 28'	23° 20'	23° ...	23° ...	23° 26'
2	9	10	14	20	21	21	...	...	...	14
3	7	8	11	20	23	24	...	11	10	10
4	9	9	16	22	23	23	...	...	...	12
5	9	10	16	22	23	22	16	11	...	...
6	6	6	15	23	24	23	...	14	...	8
7	7	8	...	26	27	26	18	13	...	8
8	23	23	18	23	23	23	...	16	13	...
9	7	9	...	24	26	26	20	17	...	15
10	15	16	...	28	28	26	...	...	...	14
11	9	14	...	25	25	24	...	17	14	14
12	3	...	15	26	18	...	...	17	17	16
13	5	6	...	25	26	23	21	...	...	13
14	4	...	...	19	22	26	...	...	15	14
15	8	...	...	20	23	23	19	14	13	13
16	8	10	...	20	22	22	...	...	12	9
17	8	...	15	24	28	28	...	16	...	10
18	6	7	13	19	19	20	...	18	15	14
19	5	...	11	18	23	23	20	18	15	15
20	8	...	11	21	23	24	16	15	14	12
21	12	...	14	19	23	23	15	15	14	14
22	11	...	14	20	23	23	18	16	13	13
23	5	...	8	22	22	23	...	...	12	11
24	3	...	11	20	23	24	20	15	12	11
25	7	9	15	20	23	23	21	17	11	10
26	5	...	14	21	24	26	22	14	11	10
27	8	10	17	24	26	27	21	14	10	9
28	5	6	13	18	19	19	20	18	15	5
29	10	...	19	27	28	25	16	...	...	16
30	9	...	21	24	24	24	20	16	21	10

TABLE I.

Observations on the Variation of the magnetic Needle.

1786	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Oct. 1	23 7	23 9	23 13	23 21	23 23	23 ...	23 ...	23 ...	23 12	23 10	23 ...
2	14	...	17	23	26	27	20	11	10	10	11
3	11	...	18	23	27	29	22	19	16	14	13
4	9	...	15	23	23	24	18	18	14	...	...
5	9	...	14	23	25	25	21	17	14	12	12
6	9	...	14	24	24	24	21	14	14	10	11
7	8	...	11	20	23	23	19	17	15	15	15
8	9	...	15	23	23	22	17	16	16	16	16
9	8	...	14	20	23	24	20	17	...	15	15
10	10	...	15	26	26	26	20	17	...	...	14
11	10	...	12	23	25	26	23	18	16	15	15
12	7	8	13	23	25	25	20	17	15	14	14
13	8	8	15	23	27	31	29	24	19	...	...
14	10	10	13	19	21	23	21	20	19	18	17
15	10	10	11	21	23	23	21	...	18	18	...
16	8	...	13	28	31	33	...	...	17	16	16
17	10	11	14	20	22	23	19	19	18	18	...
18	...	23	20	32	30	30	20	18	16	14	13
19	17	16	20	31	34	29	21	18	17	17	...
20	15	...	23	33	34	26	25	19	17	17	...
21	14	...	15	32	28	27	21	18	15	15	...
22	10	...	16	23	23	23	19	17	16	16	...
23	10	10	19	29	29	29	21	18	17	16	...
24	10	10	12	20	24	25	23	18	16	15	...
25	8	11	15	29	35	32	31	24	15	12	11
26	11	11	15	24	24	24	21	17	...	11	...
27	10	10	14	22	25	25	18	15	13	...	...
28	16	...	21	25	25	25	17	16	15	14	14
29	10	11	14	24	25	24	20	17	16	15	...
30	14	...	17	25	30	31	26	20	16	14	...
31	10	...	14	23	25	26	19	16	14	14	14

Table I.

Observations on the Variation of the magnetic Needle.

1786	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
Nov. 1	23° 13′	23° 13′	23° 19′	23° 32′	23° 34′	23° ...	23° ...	23° ...	23° ...	23° ...	23° ...
2	10	...	11	19	21	20	19	17	15	14	...
3	10	10	15	23	22	21	21	20	16	16	...
4	10	11	14	18	20	21	19	18	15	15	15
5	10	10	16	22	23	22	19	17	17	17	17
6	12	13	15	21	22	22	20	18	18	18	14
7	10	...	15	21	23	23	21	18	17	16	...
8	11	...	17	25	25	25	22	19	16	13	...
9	13	...	16	23	24	24	20	17	...	13	13
10	11	11	14	21	22	22	20	16	15	13	13
11	11	11	15	21	21	20	17	16	15	15	15
12	11	11	15	20	21	20	17	...	...	15	15
13	13	13	17	23	21	23	21	18	16	15	15
14	11	11	14	20	20	21	20	18	16	15	15
15	16	...	22	24	23	22	20	17	16	15	15
16	10	...	11	19	19	20	19	17	...	16	...
17	13	...	13	18	20	19	20	18	16	17	...
18	15	...	14	20	20	22	22	18	16	14	...
19	12	13	13	23	24	24	22	17	15	17	...
20	11	11	13	18	23	23	22	18	14	13	15
21	12	...	18	21	21	21	21	19	16	16	...
22	13	...	18	21	23	23	21	17	16	15	15
23	13	...	16	22	24	24	23	18	...	14	...
24	13	14	14	19	21	21	19	16	14	14	...
25	14	14	15	21	22	22	21	18	17	16	16
26	...	14	15	22	23	23	20	18	16	15	15
27	15	15	19	23	24	24	20	...	16	16	...
28	...	14	15	23	...	...	...	...	17	16	...
29	15	15	16	22	23	23	21	18	16	15	...
30	14	14	15	...	...	...	21	...	17	15	15

TABLE I.

Observations on the Variation of the magnetic Needle.

1786	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Dec. 1	23 15	23 16	23 23	23 23	23 23	23 20	23 17	23 15	23 15	23 ...
2	14	14	17	19	19	19	17	15	14	...
3	13	13	19	19	20	19	17	17	14	14
4	13	11	17	19	20	19	19	17	16	16
5	11	14	19	19	19	19	17	14	13	13
6	11	13	22	...	...	...	19	17	16	16
7	11	14	18	20	20	18	16	...	...	...
8	14	14	18	19	20	19	16	14	15	...
9	13	14	18	20	20	19	18	16	16	...
10	14	15	18	...	20	...	...	15	14	...
11	14	16	19	19	20	19	16	15	14	...
12	14	15	18	20	20	18	17	15	14	14
13	14	14	18	21	22	20	19	15	14	...
14	16	17	21	23	23	21	18	...	...	...
15	17	18	23	24	24	22	...	18	16	...
16	18	19	23	24	24	19	19	17	15	15
17	14	15	23	24	25	17	17	16	15	...
18	14	20	24	26	26	23	...	16	16	...
19	17	19	22	23	23	...	...	14	13	13
20	12	19	21	23	23	21	18	15	16	...
21	16	24	21	22	22	19	15	...	...	...
22	23	24	26	27	25	23	...	17	16	...
23	16	15	19	19	20	19	16	15	15	15
24	13	15	21	23	23	22	18	17	17	17
25	17	17	24	25	25	21	16	13	14	...
26	14	16	18	22	23	21	20	19	18	18
27	14	17	23	24	24	...	...	...	16	16
28	14	14	21	23	23	...	...	...	14	14
29	14	16	22	23	23	20	16	15	14	14
30	14	15	20	22	23	21	17	15	14	14
31	15	15	22	24	24	21	20	19	16	...

Table I.

Observations on the Variation of the magnetic Needle.

1787	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Jan. 1	23 ...	23 15	23 19	23 23	23 24	23 24	23 21	23 16	23 ...	23 ...	23 ...
2	...	15	16	20	23	23	20	17	15	15	...
3	14	14	15	20	21	21	19	...	14	14	14
4	15	15	16	19	21	23	20	18	17	16	17
5	14	14	15	19	24	24	22	...	15	14	14
6	...	12	13	20	22	29	29	...	14	14	14
7	...	14	15	22	26	24	...	19	...	...	...
8	14	14	16	21	24	24	21	...	17	15	16
9	15	15	18	21	23	23	19	19	13	13	14
10	...	16	22	23	24	22	20	17	17	16	...
11	...	17	21	23	23	22	20	19	...	...	...
12	...	19	20	20	25	28	23	...	...	16	...
13	14	14	16	20	22	22	21	18	15	11	...
14	...	14	20	28	28	28	25	19	14	13	14
15	...	15	21	23	23	23	20	17	14	14	...
16	...	13	16	23	23	22	19	14	14	13	...
17	...	14	15	24	25	27	23	20	16	15	...
18	...	13	14	21	25	25	20	16	...	...	...
19	...	15	16	22	24	24	22	17	15	15	...
20	...	14	15	24	24	24	21	17	14	15	...
21	...	14	16	21	23	23	20	20	18	16	...
22	...	15	16	21	23	24	21	16	14	14	...
23	...	14	21	25	29	26	24	18	16	13	...
24	...	13	18	23	25	26	24	19	18	14	...
25	...	14	19	22	27	30	28	26	...	...	...
26	14	14	22	27	28	29	22	22	21	21	...
27	...	14	16	24	24	24	20	15	10	9	...
28	...	10	16	21	24	24	23	18	15	14	14
29	13	14	16	24	24	24	23	23	20	18	17
30	14	14	15	22	24	25	24	21	18	15	15
31	13	14	15	24	24	24	23	18	15	13	14

Table I.

Observations on the Variation of the magnetic Needle.

1787	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.	12 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Feb. 1	23 13	23 12	23 15	23 21	23 23	23 23	23 21	23 20	23 ...	23 17	23 ...	23 17
2	16	16	17	20	20	20	21	19	19	18	...	...
3	16	16	21	22	24	25	31	27	19	14	14	...
4	16	16	19	25	26	26	23	18	16	16	...	...
5	16	16	17	22	25	25	24	18	16	14	14	...
6	14	14	16	23	23	24	23	21	16	10	...	...
7	...	16	18	27	26	24	24	17	16	10	...	9
8	...	17	16	21	25	25	20	16	...	14	...	...
9	14	13	15	23	23	23	23	21	17	16	...	16
10	17	17	18	22	25	25	22	17	15	11	11	13
11	13	14	15	23	27	26	21	15	11	10	10	...
12	15	16	14	23	22	23	21	19	18	16	16	...
13	13	12	16	25	25	23	20	16	13	12	12	...
14	11	11	14	22	23	23	20	...	...	...	...	...
15	13	15	17	24	31	30	28	20	...	10	...	...
16	...	16	17	24	25	25	23	21	18	17	...	...
17	...	28	28	28	23	28	20	18	...	17	...	...
18	...	16	17	22	24	24	24	19	16	16	16	...
19	9	9	9	20	22	22	21	18	16	15	...	...
20	...	12	12	25	27	28	29	20	15	10	...	...
21	...	21	23	26	26	26	25	...	16	13	...	12
22	12	11	19	21	24	23	23	16	...	...	...	...
23	15	15	21	...	...	31	29	...	11	10	10	...
24	14	14	17	23	26	29	25	18	14	13	13	...
25	13	13	15	28	29	30	29	25	...	23	...	...
26	23	23	23	24	24	23	...	19	16	12	12	...
27	12	12	17	24	25	26	24	18	14	12	...	...
28	14	13	14	21	22	22	...	15	9	14	...	13

Table I.

Observations on the Variation of the magnetic Needle.

1787.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M	4 P.M.	6 P.M.	8 P.M.	10 P.M	11 P.M.	12 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Mar. 1	23 13	23 13	23 17	3 25	23 27	23 28	23 22	23 18	23 ...	23 11	23 ...	23 ...
2	11	11	15	23	24	24	20	...	...	...	12	12
3	19	19	16	26	27	26	24	23	13	11	11	...
4	12	12	15	29	28	29	26	20	15	15	15	...
5	11	12	14	19	25	26	23	16	17	17	...	...
6	10	11	16	26	26	26	21	22	5	12	...	...
7	14	13	15	26	26	26	25	23	19	7	8	...
8	12	13	12	25	26	26	..	...	...	...	...	...
9	13	12	14	28	29	29	...	21	19	18	...	...
10	15	15	16	26	27	27	20	18	18	18	...	...
11	12	13	14	...	26	27	...	...	17	16	...	...
12	12	11	14	24	28	28	24	20	11	13	...	...
13	17	17	18	31	31	31	21	14	12	12	13	...
14	12	12	20	26	26	26	18	18	18	18	19	...
15	19	19	22	28	28	27	19	18	...	...	...	...
16	12	12	15	25	26	26	19	18	18	16	...	19
17	13	15	19	26	27	26	22	17	19	18	18	...
18	8	8	11	31	33	33	22	20	16	...	...	...
19	9	8	11	26	28	28	21	16	14	15	...	1
20	9	9	12	31	30	32	23	17	14	13	...	13
21	28	28	31	29	29	29	27	33	10	10	...	11
22	12	11	16	31	33	33	26	20	19	19	18	...
23	12	13	14	26	28	27	22	19	17	19	18	...
24	13	13	14	28	28	26	22	19	18	18	17	...
25	11	11	13	25	26	28	21	17	18	19	17	...
26	12	12	12	25	27	28	23	19	19	19	19	...
27	15	14	14	28	28	27	22	19	18	17	17	...
28	13	13	12	27	26	26	22	17	18	18	...	...
29	8	8	13	26	32	33	25	14	...	16	16	...
30	11	10	14	25	28	29	23	19	17	16	17	...
31	8	8	13	23	27	26	24	18	15	17	16	...

TABLE I.

Observations on the Variation of the magnetic Needle.

1787	6 A.M.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Apr. 1	23 ...	23 12	23 12	23 16	23 24	23 26	23 28	23 26	23 21	23 19	23 19	23 18
2	...	7	7	20	32	35	37	36	...	...	...	...
3	...	15	10	18	23	24	24	22	15	17	16	...
4	...	9	10	15	24	25	25	22	16	11	16	17
5	...	10	10	15	25	26	26	22	19	18	18	18
6	...	14	14	16	23	30	30	23	18	13	15	13
7	...	9	9	12	23	28	29	24	19	13	12	13
8	...	10	10	12	23	29	30	24	19	15	15	15
9	...	8	8	15	26	30	31	29	22	18	17	17
10	8	7	8	14	23	27	28	26	20	16	16	16
11	8	7	8	12	21	28	29	24	18	15	15	16
12	...	7	7	10	17	27	28	23	16	12	11	12
13	...	11	11	13	19	26	27	23	17	16	17	16
14	...	8	8	13	21	23	28	19	15	14	14	14
15	9	8	8	13	23	30	29	19	16	14	13	13
16	18	18	18	21	29	30	...	...	16	16	17	...
17	...	8	8	13	23	26	27	21	15	15	15	...
18	...	7	8	12	24	26	26	23	20	15	15	15
19	9	8	10	13	23	26	25	19	20	...	...	...
20	6	10	6	11	22	28	29	21	18	17	17	17
21	12	11	11	13	23	26	27	26	25	19	18	18
22	...	15	15	15	20	21	24	24	17	17	17	17
23	7	8	6	13	23	24	24	20	15	16	17	16
24	11	13	11	15	25	28	26	25	17	17	16	16
25	...	9	9	10	27	26	26	17	17	18	17	18
26	...	8	9	11	24	27	26	21	20	...	...	...
27	...	11	10	19	30	32	32	17	18	18	17	...
28	9	10	12	12	23	23	23	17	15	15	13	14
29	...	7	6	9	21	25	25	21	17	15	...	...
30	...	13	13	15	25	28	27	20	15	14	15	14

TABLE I.

Observations on the Variation of the magnetic Needle.

1787	6 A.M.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
May 1	23° 8′	23° 8′	23° 8′	23° 16′	23° 23′	23° 26′	23° 26′	23° 22′	23° 16′	23° 16′	23° 15′	23° 16′
2	8	8	8	15	25	25	25	21	20	19	18	17
3	11	11	11	14	23	27	26	21	15	...	...	...
4	6	5	6	13	28	29	29	23	17	16	16	16
5	7	8	7	11	25	26	26	21	17	15	15	16
6	9	7	7	17	28	27	28	25	18	17	18	17
7	9	8	8	13	25	28	28	21	19	19	9	14
8	7	8	7	17	25	26	26	22	17	13	15	14
9	...	...	...	18	30	30	30	26	20	19	19	20
10	2	4	3	23	32	30	27	18	18	...	17	26
11	6	6	6	11	24	23	24	20	17	17	16	17
12	9	8	8	11	23	24	24	20	17	16	17	17
13	6	5	5	11	24	25	25	...	...	15	14	13
14	11	10	6	10	38	38	31	27	26	18	17	17
15	13	11	11	13	25	26	26	16	19	18	17	19
16	8	8	9	14	22	25	25	23	24	23	26	24
17	10	11	10	13	17	21	21	17	17	...	...	...
18	8	9	9	13	28	29	29	24	15	15	15	15
19	4	3	3	9	22	25	25	24	16	14	16	...
20	9	7	7	11	23	25	25	19	19	19	19	18
21	10	10	11	14	28	30	27	26	23	22	22	21
22	10	11	10	13	26	27	27	21	15	14	16	15
23	10	9	9	14	24	26	26	21	15	16	16	17
24	6	6	6	12	24	25	25	16	16	16	...	...
25	7	7	7	16	25	26	27	21	15	17	16	16
26	5	5	7	14	24	25	25	19	17	17	17	17
27	7	7	8	14	23	26	26	...	...	17	17	17
28	6	5	5	10	23	25	25	20	17	17	17	17
29	8	8	10	13	24	26	26	19	19	19	18	19
30	5	6	6	14	26	27	27	19	16	16	16	17
31	4	5	4	11	24	26	27	19	16	18	15	17

TABLE I.

Observations on the Variation of the magnetic Needle.

1787	6 A.M	7 A.M.	8 A.M.	10 A.M	12 M.	1 P. M.	2 P. M.	4 P. M.	6 P. M	8 P. M.	10 P M	11 P. M.
June 1	23° 8′	23° 7′	23° 7′	23° 11′	23° 26′	23° 28′	23° 28′	23° 22′	23° 17′	23° 16′	23° 18′	23° 17′
2	6	6	6	17	24	25	25	20	19	17	18	17
3	6	6	6	18	28	28	27	24	20	18	19	18
4	6	5	7	13	24	26	25	21	19	16	15	16
5	6	6	5	16	29	28	29	22	19	17	16	17
6	7	6	8	17	29	30	30	24	19	19	16	15
7	5	5	5	13	25	30	30	27	22	17	..	16
8	7	8	8	19	28	28	28	27	20	16	15	16
9	18	17	19	21	26	26	26	19	17	17	17	17
10	16	15	16	20	24	26	26	22	19	18	19	19
11	6	7	6	16	28	27	27	20	19	19	18	19
12	6	6	6	15	28	29	29	26	19	19	19	19
13	6	5	6	13	25	27	27	23	20	19	19	19
14	6	6	7	14	26	30	31	24	20	16	...	16
15	3	5	5	10	28	31	32	25	20	20	20	20
16	6	5	6	15	28	31	33	26	17	17	16	18
17	8	8	10	11	28	31	30	25	18	19	16	17
18	9	8	10	16	29	31	31	26	18	18	18	17
19	6	5	5	13	26	27	27	24	17	17	18	17
20	14	13	14	21	31	32	32	24	19	17	16	18
21	8	9	8	18	26	29	29	24	19	19	...	18
22	11	11	10	18	29	29	29	24	19	18	19	18
23	8	8	10	18	26	26	26	20	19	19	19	19
24	6	6	6	16	23	28	29	22	19	19	19	19
25	19	19	19	19	28	28	29	21	19	18	18	18
26	8	7	7	15	23	23	23	18	18	19	18	18
27	9	11	9	14	28	30	29	18	18	18	18	18
28	10	9	11	19	26	28	26	19	16	17	...	...
29	9	9	13	18	26	26	25	20	18	17	19	18
30	10	8	10	17	24	26	26	21	18	20	18	18

Table I.

Observations on the Variation of the magnetic Needle.

1787	6 A.M	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
July 1	23 9	23 9	23 11	23 18	23 26	23 26	23 25	23 19	23 16	23 17	23 16	23 16
2	9	10	9	15	29	29	28	22	17	16	17	17
3	5	5	7	12	27	27	27	22	17	17	16	17
4	8	7	11	17	28	30	29	22	19	16	16	17
5	8	7	8	11	28	30	30	21	17	17	18	17
6	7	7	8	22	25	27	27	21	19	20	18	19
7	8	6	7	16	27	28	28	22	18	18	18	18
8	6	7	8	15	25	27	27	21	18	18	18	19
9	7	8	7	15	25	27	27	22	20	20	20	20
10	5	5	5	15	24	24	24	23	20	20	20	20
11	7	7	8	17	26	27	27	26	23	19	19	19
12	5	5	7	14	30	33	33	23	21	20	19	19
13	10	10	10	17	30	30	30	24	19	19	19	20
14	10	10	11	19	28	29	30	23	20	17	18	18
15	11	11	13	20	28	29	29	22	20	18	21	18
16	10	11	13	18	25	29	27	24	23	23	23	23
17	8	6	8	21	30	35	36	26	18	19	20	19
18	9	9	11	14	25	29	29	26	19	...	18	19
19	9	8	11	19	25	29	29	25	20	19	20	20
20	9	9	10	20	28	29	29	25	20	21	20	20
21	7	8	8	20	29	30	30	25	19	18	20	19
22	8	10	8	19	29	30	30	24	20	18	22	19
23	15	17	16	16	28	30	30	26	23	22	23	22
24	8	8	8	16	28	31	33	27	21	21	22	21
25	13	16	14	18	28	31	33	20	20	20	20	20
26	14	14	14	15	26	30	31	24	21	21	22	21
27	14	14	15	22	...	...	...	...	...	...	20	20
28	14	15	14	26	30	30	31	23	19	19	21	20
29	16	16	16	21	31	32	31	20	18	18	19	19
30	13	12	13	23	30	30	32	26	19	18	19	18
31	12	11	12	22	29	30	30	...	...	...	...	...

Table I.

Observations on the Variation of the magnetic Needle.

1787	6 A.M.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Aug. 1	23 8	23 9	23 13	23 19	23 38	23 39	23 39	23 36	23 23	23 18	23 17	23 17
2	7	8	17	21	31	31	31	24	19	19	18	19
3	9	8	13	18	28	34	35	24	19	18	19	17
4	11	13	12	18	31	31	31	23	19	19	21	19
5	13	12	11	15	28	33	32	26	...	...	19	20
6	11	10	12	17	26	31	33	27	21	21	21	20
7	10	11	10	16	26	30	33	28	21	20	20	21
8	9	9	10	17	30	32	32	29	21	21	19	21
9	11	13	12	17	30	31	30	25	19	19	19	20
10	13	12	14	22	31	33	34	29	18	16	19	16
11	10	14	11	20	32	32	32	26	15	15	15	17
12	15	13	14	20	28	31	30	25	19	19	19	19
13	8	11	10	17	29	30	30	24	19	19	19	19
14	10	10	10	20	30	31	31	23	19	18	17	16
15	10	10	9	19	38	38	35	29	20	19	19	20
16	13	14	12	21	33	35	33	29	19	19	19	19
17	8	7	9	18	29	30	30	25	20	19	18	18
18	7	7	8	21	28	29	29	28	21	19	17	19
19	8	8	9	20	31	...	...	...	...	...	19	19
20	21	21	20	29	35	34	31	27	19	19	21	20
21	21	20	21	29	34	33	32	24	19	21	19	20
22	13	13	14	17	27	29	29	25	20	18	19	19
23	13	12	13	17	29	30	30	24	19	17	18	18
24	11	11	13	17	29	30	29	23	21	19	21	22
25	21	20	21	25	31	31	31	22	19	19	20	19
26	12	14	13	18	28	30	30	...	18	19	19	18
27	14	14	15	21	29	29	29	21	17	18	18	18
28	12	12	13	18	29	30	30	25	21	19	21	...
29	14	14	14	21	30	31	32	25	18	18	18	18
30	12	11	12	19	29	31	31	22	19	18	19	17
31	13	12	13	23	33	31	31	23	19	18	18	20

TABLE I.

Observations on the Variation of the magnetic Needle.

1787	6 A.M.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
Sep. 1	23° 13′	23° 14′	23° 14′	23° 23′	23° 33′	23° 34′	23° 35′	23° 26′	23° 20′	23° 21′	23° 20′	23° 20′
2	13	13	14	21	31	32	32	...	21	20	20	20
3	14	14	14	22	33	33	32	26	21	20	21	19
4	13	13	15	19	29	31	31	25	19	21	20	20
5	14	13	14	18	29	30	30	26	21	17	19	19
6	14	14	15	21	29	31	31	28	21	19	19	21
7	19	19	20	26	33	35	34	31	21	16	17	17
8	15	16	15	21	28	30	29	24	18	17	17	17
9	15	16	16	21	30	30	29	...	20	18	19	19
10	14	14	14	22	30	30	30	23	19	18	18	18
11	13	14	14	21	29	29	29	22	19	20	19	18
12	12	11	14	18	30	30	29	23	18	18	19	19
13	13	15	14	18	29	30	30	22	19	19	19	19
14	14	15	15	21	30	30	30	25	20	19	18	20
15	15	14	14	20	32	33	33	26	19	19	20	19
16	13	13	15	21	36	35	36	23	18	19	19	18
17	16	14	20	18	28	36	36	...	23	19	20	20
18	19	20	15	23	35	31	33	26	22	21	21	20
19	16	16	16	21	27	29	29	...	...	20	19	20
20	16	18	14	23	27	29	28	23	21	19	20	20
21	16	16	14	21	28	30	28	24	21	20	18	19
22	17	17	13	22	29	28	28	23	21	20	19	20
23	16	16	14	21	28	29	29	21	18	18	18	18
24	18	17	15	22	29	31	31	26	20	20	21	19
25	18	19	14	26	31	32	29	25	21	20	20	21
26	17	17	13	21	28	27	27	21	17	16	16	16
27	16	16	14	20	29	30	30	26	22	18	20	19
28	15	14	19	18	26	28	27	22	19	19	19	19
29	13	13	17	20	27	28	28	26	22	21	21	21
30	14	13	17	16	31	30	31	29	23	21	21	22

TABLE I.

Observations on the Variation of the magnetic Needle.

1787	7 A.M	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Oct. 1	23 17	23 17	23 23	23 32	23 32	23 32	23 29	23 21	23 21	23 21	23 ...
2	19	19	19	30	30	30	26	21	20	20	...
3	18	18	24	30	33	33	23	21	21	21	21
4	14	14	21	30	33	33	26	21	21	12	...
5	18	17	25	31	35	34	31	17	13	14	13
6	15	15	19	29	31	31	27	21	19	19	20
7	15	15	...	30	31	31	27	22	21	21	...
8	15	15	22	30	30	30	26	20	20	20	...
9	14	14	19	29	31	32	27	21	20	21	...
10	15	16	21	33	34	34	30	21	21	21	...
11	19	17	26	33	34	33	...	25	21	19	...
12	16	17	23	30	31	30	26	23	22	23	...
13	18	16	18	29	29	29	...	...	...	...	...
14	21	21	27	31	32	32	...	22	20	20	...
15	16	15	19	28	31	31	28	21	22	20	...
16	16	16	18	27	28	28	27	24	24	22	...
17	15	18	16	26	26	26	26	23	24	21	...
18	17	17	17	28	26	31	27	21	20	20	...
19	17	16	21	26	28	28	27	24	22	22	...
20	17	17	18	29	29	29	26	19	20	19	...
21	16	16	18	29	30	30	...	22	19	21	...
22	18	17	18	31	33	32	...	...	20	20	...
23	17	16	19	30	30	29	23	21	19	20	...
24	18	19	20	33	33	31	33	23	21	17	...
25	23	18	23	37	38	37	34	23	23	22	...
26	21	22	26	35	37	35	28	22	24	25	...
27	26	28	27	35	35	33	28	22	21	22	...
28	19	18	24	34	35	33	...	...	21	20	21
29	18	18	21	34	35	35	24	21	22	21	...
30	18	18	23	35	35	35	28	22	22	21	22
31	16	15	19	32	34	34	29	29	...	...	...

Table I.

Observations on the Variation of the magnetic Needle.

1787	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Nov. 1	23 21	23 20	23 21	23 30	23 30	23 28	23 27	23 22	23 18	23 20	23 ...
2	17	20	20	28	31	28	26	22	20	22	23
3	20	20	21	27	37	35	31	22	23	22	...
4	18	18	...	35	36	35	28	21	22	22	21
5	17	18	21	35	35	35	32	24	21	18	...
6	18	18	19	35	35	33	30	22	21	20	20
7	17	17	19	30	33	33	26	20	22	20	21
8	17	17	19	30	33	33	26	20	22	20	21
9	17	18	20	30	33	33	31	31	...	22	...
10	18	17	19	31	33	33	31	28	22	22	23
11	20	20	21	31	34	35	32	...	20	22	22
12	18	20	19	28	31	32	28	21	22	21	...
13	18	19	19	28	29	30	28	22	21	21	22
14	18	18	20	28	29	29	25	22	21	22	22
15	20	19	20	28	28	28	27	25	...	23	23
16	19	19	20	27	26	28	26	19	17	20	20
17	20	19	20	26	28	27	25	21	20	21	20
18	19	19	20	29	29	29	...	25	24	...	...
19	22	23	23	31	32	31	31	26	22	22	21
20	23	22	23	31	33	32	29	22	21	21	22
21	21	23	23	28	29	29	26	22	21	22	23
22	21	20	21	31	29	30	29	23	...	22	22
23	21	21	21	29	28	29	26	22	23	22	...
24	22	20	21	30	31	30	26	22	22	20	21
25	22	22	22	30	30	30	...	...	22	21	...
26	20	21	21	30	29	31	26	22	23	...	...
27	21	22	21	28	30	29	26	23	23	22	...
28	19	21	20	26	27	27	26	23	22	23	...
29	21	20	21	31	31	29	25	23	22	22	...
30	22	21	23	31	34	33	28	22	21	22	...

TABLE I.

Observations on the Variation of the magnetic Needle.

1787	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Dec. 1	23 21	23 21	23 20	23 29	23 31	23 30	23 28	23 22	23 23	23 22	23 22
2	20	21	21	31	31	31	27	23	22	21	22
3	21	20	21	30	31	29	26	22	22	23	...
4	21	22	22	30	31	31	...	...	22	21	...
5	18	20	21	28	31	26	26	22	21	23	...
6	22	22	23	29	29	30	28	24	23	21	...
7	21	21	22	28	30	29	26	23	23	22	22
8	20	21	21	26	28	29	29	26	22	22	...
9	22	20	21	26	31	30	26	26	21	22	...
10	21	21	21	29	29	29	25	23	21	22	21
11	18	21	22	28	29	31	28	21	23	21	...
12	20	20	21	33	33	33	31	24	24	19	...
13	...	21	21	28	30	30	27	25	...	22	...
14	21	21	26	30	31	33	...	...	...	22	...
15	20	22	22	30	31	30	27	21	23	20	...
16	...	22	29	31	28	31	...	25	22	22	...
17	...	22	23	31	33	33	...	23	22	23	21
18	...	21	20	28	29	29	28	23	24	22	...
19	...	19	22	29	28	27	25	23	22	23	...
20	...	21	24	28	30	29	26	24	24	24	...
21	...	23	26	29	29	29	25	22	22	22	...
22	...	21	21	24	26	24	22	25	20	19	...
23	...	19	21	24	24	24	...	20	20	19	...
24	...	20	20	29	30	29	24	24	22	22	...
25	...	22	20	28	28	28	26	22	20	21	...
26	...	22	22	29	33	29	26	22	23	22	...
27	...	21	21	24	26	26	23	21	21	21	...
28	...	21	21	25	25	26	24	23	20	21	...
29	...	20	20	23	22	25	25	20	20	21	...
30	...	21	21	...	25	30	29	...	...	...	...
31	...	21	20	29	26	28	25	22	22	...	...

Table II.

Mean monthly Variation of the magnetic Needle.

1786	6 A.M.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Sept.	23	23 7,9	23 10,1	23 14,5	23 22,2	23 23,7	23 23,9	23 19,0	23 15,3	23 13,5	23 12,4	23 ....
Oct.	. . .	10,4	11,3	15,2	24,4	26,1	26,1	21,1	17.7	15,6	14,5	13,8
Nov.	. . .	12,2	12,5	15,3	21,6	22,5	22,0	20,3	17,6	15,9	15,1	14,7
Dec.	. . .	. . .	14,5	16,1	20,6	22,0	22,2	20,0	17,4	15,8	15,0	15,0
1787												
Jan	. . .	14,0	14,2	17,1	22,3	24,1	24,5	21,8	18,4	15,6	14,5	14,8
Feb.	. . .	14,2	15,1	17,1	23,3	24,8	25,1	23,6	18,8	15,3	15,8	12,8
Mar.	. . .	12,8	12,8	15,3	26,5	27,7	27,8	18,4	19,0	15,9	15,5	15,7
April	9,7	9,9	9,7	13,9	23,6	27,0	27,4	22,6	17,8	15,7	15,7	15,6
May	7,6	7,5	7,4	13,5	25,2	26,6	26,2	21,0	17,7	17,1	16,8	17,0
June	8,4	8,2	8,8	16,0	26,6	28,1	28,1	22,6	18,7	17,9	17,8	17,7
July	9,5	9,6	10,3	17,8	27,6	29,3	29 4	23,2	19,4	18,9	19,3	19,1
Aug.	11,9	12,0	12,8	19,7	30,3	31,7	31,5	25,6	19,3	18,7	18,9	18,8
Sept.	15,0	15,1	15,3	20,2	29,8	30,7	30,5	24,7	20,1	19,1	19,2	19,2
Oct.	. . .	17,5	17,3	21,1	30,8	31,9	31,6	27,4	21,9	20,8	20,2	19,6
Nov.	. . .	19,4	19,7	20,6	29,7	31,1	30,2	27,7	22,7	21,4	21,3	21,4
Dec.	. . .	20,4	21,0	21,8	28,2	29,0	29,0	26,2	22,9	21,9	21,6	. . .

TABLE III.

Mean monthly true Variation, and mean monthly diurnal Alteration of Variation of the magnetic Needle.

	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.
	January.		February.		March.		April.		May.		June.	
	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′
1786	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
1787	23 19,2	10,2	23 19,8	10,4	23 20,3	15,0	23 18,5	17,4	23 17,0	18,9	23 18,3	19,6
1788	23 25,6	8,7	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	23 28,9	18,8
1789	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	23 34,2	17,1
1790	23 38,9	8,4	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
1791	23 35,6	6,8	. . .	. . .	. . .	. . .	23 36,0	15,0	. . .	. . .	. . .	. . .
1792	23 41,1	5,4	. . .	. . .	. . .	. . .	. . .	. . .	23 41,9	11,5	. . .	. . .
1793	23 46,9	4,3	23 48,3	4,6	23 48,8	8,5	23 46,2	11,7	23 47,3	10,4	23 48,5	12,6
1794	23 54,2	4,5	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
1795	. . .	. . .	. . .	. . .	23 57,5	9,8	. . .	. . .	. . .	. . .	23 57,1	9,4
1796	. . .	. . .	. . .	. . .	24 1,1	7,0	. . .	. . .	. . .	. . .	23 58,7	9,8
1797	. . .	. . .	. . .	. . .	24 1,5	7,4	. . .	. . .	. . .	. . .	24 0,2	11,6
1798	. . .	. . .	. . .	. . .	24 0 6	7,2	. . .	. . .	. . .	. . .	23 59,4	11,2
1799	. . .	. . .	. . .	. . .	24 1,1	7,5	. . .	. . .	. . .	. . .	24 0,6	10,8
1800	. . .	. . .	. . .	. . .	24 3,6	6,9	. . .	. . .	. . .	. . .	24 1,8	10,9
1801	. . .	. . .	. . .	. . .	24 5,2	8,8	. . .	. . .	. . .	. . .	24 2,8	10,8
1802	. . .	. . .	. . .	. . .	24 6,9	9,5	. . .	. . .	. . .	. . .	24 5,3	10,7
1803	. . .	. . .	. . .	. . .	24 8,0	11,8	. . .	. . .	. . .	. . .	24 7,0	12,6
1804	. . .	. . .	. . .	. . .	24 9,4	10,0	. . .	. . .	. . .	. . .	24 6,0	11,3
1805	. . .	. . .	. . .	. . .	24 8,7	8,1	. . .	. . .	. . .	. . .	24 7,8	12,5

Table III.

Mean monthly true Variation, and mean monthly diurnal Alteration of Variation of the magnetic Needle.

	July.		August.		September.		October.		November.		December.	
	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.
	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′
1786	. . .	. . .	. . .	. . .	23 16,4	14,8	23 18,4	15,3	23 17,3	9,9	23 18,3	7,6
1787	23 19,6	19,6	23 21,9	19,4	23 22,8	15,5	23 24,5	14,3	23 25,0	11,1	23 25,8	8,3
1788	23 29,8	16,4	. . .	. . .	. . .	. . .	23 32,1	14,6	. . .	. . .	. . .	. . .
1789	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	23 41,2	5,4
1790	23 39,0	15,4	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
1791	23 36,7	15,2	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
1792	. . .	. . .	23 43,6	12,7	23 43,9	11,1	23 45,6	8,9	23 45,9	3,7	23 45,2	3,1
1793	23 50,5	12,5	23 48,6	12,1	23 52,6	9,8	23 52,3	7,0	23 51,9	3,8	23 52,3	3,8
1794	23 54,4	11,2	23 57,2	9,8	23 58,1	8,4	. . .	. . .	. . .	. . .	. . .	. . .
1795	23 57,1	8,4	. . .	. . .	24 0,4	7,6	. . .	. . .	. . .	. . .	23 59,4	3,6
1796	23 59,2	10,1	. . .	. . .	24 0,1	8,3	. . .	. . .	. . .	. . .	24 1,3	4,9
1797	24 0,3	10,1	. . .	. . .	24 1,4	7,6	. . .	. . .	. . .	. . .	24 1,3	5,0
1798	24 0,0	10,0	. . .	. . .	24 1,4	9,4	. . .	. . .	. . .	. . .	24 1,4	2,7
1799	24 1,8	10,4	. . .	. . .	24 2,9	7,8	. . .	. . .	. . .	. . .	24 2,3	3,4
1800	24 3,0	9,2	. . .	. . .	24 3,6	7,7	. . .	. . .	. . .	. . .	24 3,3	3,1
1801	24 4,1	10,3	. . .	. . .	24 3,8	10,1	. . .	. . .	. . .	. . .	24 5,4	2,5
1802	24 6,0	12,3	. . .	. . .	24 8,7	8,9	. . .	. . .	. . .	. . .	24 6,8	3,8
1803	24 7,9	13,1	. . .	. . .	24 10,5	9,5	. . .	. . .	. . .	. . .	24 10,7	3,0
1804	24 8,4	10,4	. . .	. . .	24 8,9	9,3	. . .	. . .	. . .	. . .	24 9,0	3,7
1805	24 7,8	10,4	. . .	. . .	24 10,0	9,3	. . .	. . .	. . .	. . .	24 9,4	4,6

TABLE IV.

Differences between the Observations of the Variation of the magnetic Needle at the Times of the Equinoxes and those at the Solstices.

Years.	March.	June.	September.	December.
1793	+ 3′,6	− 0′,3	+ 4′,1	− 0′,3
1795	. . . .	− 0,4	+ 3,3	− 1,0
1796	+ 1,7	− 2,4	+ 1,4	+ 1,2
1797	+ 0,2	− 1,3	+ 1,2	− 0,1
1798	− 0,7	− 1,2	+ 2,0	0,0
1799	− 0,3	− 0,5	+ 2,3	− 0,6
1800	+ 1,3	− 1,8	+ 1,8	− 0,3
1801	+ 1,9	− 2,4	+ 1,0	+ 1,6
1802	+ 1,5	− 1,6	+ 3,4	− 1,9
1803	+ 1,2	− 1,0	+ 3,5	+ 0,2
1804	− 1,3	− 3,4	+ 2,9	+ 0,1
1805	− 0,3	− 0,9	+ 2,2	− 0,6
Mean	+0′,80	−1′,43	+2′,43	−0′,14

Table V.

Dip of the magnetic Needle.

Poles reversed.

	Face East.	Face West.	Face East.	Face West.	True Dip.	
1786						
September	72 28,7	72 1,4	71 57,3	72 5,1	72 8,1	Mean of 9 means.
October	72 29,9	71 59,0	72 0,4	72 1,2	72 7,6	—— 7 ——
November	72 7,6	72 17,6	72 2,4	71 46,7	72 3,6	—— 6 ——
December	72 10,6	72 2,2	72 2,2	71 58,4	72 3,4	—— 6 ——
1787						
January	72 11,4	1,8	72 1,0	71 56,0	72 2,5	—— 11 ——
February	72 19,4	10,8	72 1,5	71 55,8	72 6,9	12
March -	72 19,1	11,9	72 0,5	71 52,2	72 5,9	14
April -	72 24,4	9,5	72 0,5	71 52,2	72 6,6	14
May -	72 24,4	9,6	72 4,2	71 52,9	72 7,8	15
June -	72 22,6	7,9	72 4,2	71 52,9	72 6,8	15
July -	72 22,6	7,9	71 59,9	71 55,1	72 6,4	15
August	72 22,3	6,7	71 59,3	71 55,2	72 5,9	15
September	72 22,3	6,7	72 2,9	71 51,0	72 5,7	15
October	72 23,1	2,5	72 2,9	71 51,0	72 4,9	15
November	72 23,1	2,5	72 2,7	71 50,3	72 4,7	15
December	72 22,8	2,0	72 2,7	71 50,3	72 4,4	15
1788						
January	72 22,8	72 2,0	72 2,6	71 48,8	72 4,0	15
1789						
January	72 16,0	72 0,0	71 51,9	71 31,1	71 54,8	
December	72 17,5	71 59,4	71 38,9	71 42,8	71 54,6	4
1790						
January	72 16,9	71 57,7	71 40,2	71 40,2	71 53,7	7
1791						
January	71 43,9	71 36,1	71 37,2	71 17,5	71 23,7	4
1795						
October	71 12,8	71 9,5	71 13,9	71 9,4	71 11,4	14
1797						
October	71 4,9	71 10,9	70 56,3	70 44,7	70 59,2	30
1798						
April -	71 4,7	71 14,5	71 2,3	70 19,8	70 55,4	16
October	70 55,6	71 14,5	71 7,7	70 22,2	70 55,0	16
1799						
October	70 56,0	71 13,5	71 11,5	70 7,9	70 52,2	16
1801						
April -	70 47,4	71 5,6	70 52,4	69 38,2	70 35,6	16
1803						
October	70 30,9	71 9,9	70 40,5	69 46,7	70 32,0	16
1805						
August -	70 25,2	70 55,7	70 26,9	69 36,3	70 21,0	16

XXI. *On the Declinations of some of the principal fixed Stars; with a Description of an Astronomical Circle, and some Remarks on the Construction of Circular Instruments. By* John Pond, *Esq. Communicated by* Smithson Tennant, *Esq. F. R. S.*

Read June 26, 1806.

THE observations which accompany this Paper were made at Westbury in Somersetshire, in the years 1800 and 1801, with an Astronomical Circle of two feet and a half diameter, constructed by Mr. TROUGHTON, and considered by him as one of the best divided instruments he had ever made; a drawing of it, with a short description, is annexed to the observations. (Plate XX.)

When this instrument came into my possession, I thought I could not employ it in a more advantageous manner, than in endeavouring to determine the declinations of some of the principal fixed stars.* The various catalogues differed so much from each other, and such doubt existed as to the accuracy of those which were thought most perfect; that the declinations of few stars could be considered as sufficiently well ascertained for the more accurate purposes of astronomy.

The advantages that have resulted from the excellent method pursued at Greenwich, of observing constantly the transits of a few stars, to obtain accurately their right ascensions, induced me to follow the same method for determining

* At that time Dr. MASKELYNE's late Catalogue was not published.

their declinations; and for a considerable period I constantly observed them on the meridian, whenever they passed at a convenient hour; usually reversing the instrument in azimuth at the end of every day's observation; never considering any observation as complete that had not its corresponding one in a short interval of time. When this circumstance is not attended to, I think, a great part of the advantage arising from the circular construction is lost.

The observations themselves will show, if they have been made with the requisite care and attention to merit the notice of astronomers; for it is one of the many advantages of circular instruments, that from the observations made with them, we may infer with great precision not only the mean probable error, but likewise the greatest possible error to which they are liable. From a careful comparison of the errors of collimation, as deduced from different stars, I concluded that the greatest possible error was 2″,5, and the mean error about 1″; and by a comparison with other observations with similar instruments, it will be seen that this supposition was well founded, since nearly the same quantities are deduced by another method to be considered hereafter.

The polar distances are annexed to each observation: a method which I borrowed from Mr. Wollaston, and which is rendered very easy by employing his useful tables calculated for that purpose. This practice of reducing every day's observations cannot be too much recommended, as the labour of calculating accumulated observations is thus rendered unnecessary.

When I had deduced the declinations of these stars from my own observations, continued long enough to divest them

of all error, except that arising from defect in the divisions of the intrument, I was desirous of comparing them with the observations made by others; and I have subjoined a comparison of them with all those which I could procure, that seemed entitled to confidence. In the first column are the observations made at Greenwich, as published in 1802 by the Astronomer Royal; the second column contains a catalogue deduced from some observations made at Armagh with a very large equatorial instrument constructed by Mr. TROUGHTON. In the third column are the observations of Mr. PIAZZI, of Palermo; and in the fourth those made at Westbury. All the above mentioned observations are arranged in the order of their polar distances, and the positive deviations separated from the negative; that the cause of error in any of the instruments may be the more easily detected, as likewise any mistake in the assumed latitudes of the respective places of observation.

A general catalogue is then added; which is deduced, by taking the mean, generally of the above four; but in some places, a few detached observations that I have accidentally procured of other circular instruments have been included. The utility of this investigation is not merely confined to the determination of the polar distances of the stars; as besides this some valuable information on other points may be obtained. In the first place, upon examining the variations that appear in these observations a question naturally occurs, whether, by changing the assumed latitudes of the respective places of observation, a nearer coincidence might not be obtained. And I find, that to make the positive deviations equal to the negative, the following corrections should be applied to the co-latitudes.

Greenwich	+ 1″
Armagh	+ 1″,3
Palermo	— 1″
Westbury	— 0″,25.

This method of correcting the latitudes has, I believe, never been employed: but it seems reasonable to suppose, that when thus corrected, they will be nearer the truth, than those determined by the usual method: for the same reason, that the declinations of the stars resulting from a general comparison, are more likely to be accurate, than if deduced from any one single set of observations: but if the Greenwich instrument should be affected with any errors independent of the divisions; in that case, we should be unable to infer any thing decisive, as to the latitude, by the above method. But from a comparison of the observations of γ Draconis, observed at Greenwich and Westbury, the latitude of Westbury being previously corrected by the above method, I am inclined to believe the latitude of Greenwich requires a very small correction, certainly not exceeding a second. The result I obtain by a very careful investigation by methods, entirely independent of the Greenwich quadrant, is 51°.28′.39″,4.

I consider this comparison as interesting likewise on another account; it is an object deserving of curiosity to examine the present state of our best astronomical instruments, and to ascertain what may reasonably be expected from them. The superiority of circular instruments is, I believe, too universally admitted, to render it probable that quadrants will ever again be substituted in their place. But the Greenwich quadrant is so intimately connected with the history of astronomy, the observations that have been made with it, and the deductions

from those observations, are of such infinite importance to the science, that every circumstance relating to it cannot fail of being interesting. Now when it is considered that this instrument has been in constant use for upwards of half a century, and that the center error, from constant friction, would during this time have a regular tendency to increase, it will not appear at all surprising, if the former accuracy of this instrument should be somewhat impaired. With a view, therefore, of ascertaining more correctly the present state of an instrument on which so much depends, I have exhibited in one view the polar distances as determined by circular instruments alone; the respective co-latitudes being previously corrected by the method above mentioned, and I have compared the mean result with the Greenwich Catalogue, that the nature and amount of the deviations may be seen, and if it be judged necessary, corrected. I should add, that by some observations of the sun at the winter solstice in 1800, the difference between the Greenwich quadrant and the circle was 10 or 12″, the quadrant still giving the zenith distance too little.

General Description of the Instrument.

The annexed Plate represents the circle in its vertical position. It was originally made to be used likewise as an equatorial instrument, a circumstance I need not have mentioned, but as an apology for the slightness of its construction, which the artist who made it would not have recommended, had the instrument been intended for the vertical position only.

The declination circle, 30 inches in diameter, is composed of two complete circles; the conical radii of which are inserted

at their bases in an axis about 12 inches long, leaving sufficient space between the limbs for a telescope $3\frac{1}{2}$ feet long, and an aperture of $2\frac{3}{4}$ inches, to pass between. The two circles are firmly united at their extreme borders by a great number of bars, which stand perpendicular between them; the whole of which will be readily understood by referring to the figure. The square frames, which appear as inscribed in the circle, were added to give additional firmness to the whole.

The circle is divided by fine lines into $5'$ of a degree; and subdivided into single seconds by two micrometer microscopes, the principles and properties of which are now too well known to require any particular explanation.

At the time these observations were made, the microscopes were firmly fixed opposite to the horizontal diameter: but when I considered that, by continuing the observations, the error of division would never be diminished, I suggested to Mr. Troughton the possibility of giving a circular motion to the microscopes, though I confess with very little hope, that the thing was really practicable in an instrument previously constructed on other principles. Mr. Troughton approved of the idea, and executed it in a very ingenious manner. His talents, as an artist, are too well known and too highly appreciated, to stand in need of any praise from me; yet I should consider myself as deficient in justice, if I did not endeavour to call the attention of the reader to the skill and ingenuity, which have been employed not only in this very important alteration, but in every contrivance that is peculiar to the instrument, which is the object of our present consideration.

These microscopes can now revolve about 60° from their

horizontal position; and it is easy to comprehend, that by this valuable improvement, all errors of division may be completely done away, without any of the manifest inconveniences of the French circle of repetition; which, though a very ingenious instrument, and admirably adapted to some particular operations, will, I think, never be adopted for general use in our observatories.

The plumb-line, a very material part of this instrument, is suspended from a small hook at the top of the tube at the left hand of the figure. It passes through an angle, in which it rests in the same manner as the pivot of a transit instrument does on its support. At the lower end of the tube which protects it, a smaller tube is fixed at right angles, which contains microscopic glasses so contrived, that the image of a luminous point, like the disc of a planet, is formed on the plumb-line and bisected by it. Great attention should be given to the accurate bisection of this transparent point by the plumb-line *at the moment of observation*. It is absolutely essential in instruments of this construction to consider the observation, as consisting in two bisections *at the same time:* the one of the star by the micrometer, the other of the plumb-line-point by the plumb-line. The least negligence in either of these bisections will render the observation unsuccessful.

The two strong pillars, which support the axis of the vertical circle, are firmly united at their bases to a cross bar; to which also the long vertical axis is affixed, and which may be considered as forming one piece with them. The stone pedestal is hollow, and contains a brass conical socket, firmly fastened to the stone, and reaching almost to the ground. This socket receives the vertical axis, and supports the whole

weight of the moveable part of the instrument, which revolves on an obtuse point at the bottom; the upper part of this vertical axis is kept steady in a right angle, having two springs opposite the points of contact, which press it against its bearings, and it thus turns in these four points of contact with a very pleasant and steady motion.

The bar, in which the vertical axis is thus centered, is acted on by two adjusting screws in directions at right angles, and perfectly independent of each other. By these motions, the axis may be set as truly perpendicular, as by the usual method of the tripod with feet screws, which could not in this case have been employed.

The frame, to which this apparatus is attached, is fixed to the corners of the hexagonal stone, by the conical tubes; between which and the stone, the azimuth circle (which forms one piece with the vertical axis) turns freely. The azimuth circle of two feet diameter consists of eight conical tubes, inserted in the vertical axis; and which are united at their ends by the circular limb; this is divided and read off exactly in a similar manner to the other circle.

A level remains constantly suspended on the horizontal axis, which is verified in the same manner as in a transit instrument. There are forcing screws for this purpose, which pass through the bar on which the vertical columns stand, and these by pressing against the long axis produce a small change in the inclination of the upper part of the instrument, without altering the position of the azimuth circle or its axis.

The application of the plumb-line, as already described, is

peculiar to the instruments made by Mr. Troughton: it regards the vertical axis rather than any other part, and is, in fact, exactly analogous to the usual verification of a zenith sector.

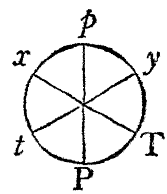
During the period in which I was engaged in making observations with circular instruments, I was led to consider the advantages and inconveniences of the usual method of adjusting them; and it appeared to me, that the essential part of their construction, which relates to their adjustment, was capable of being improved.

In order to render the nature of the improvement, which I wish to propose, more intelligible, I ought previously to remark, that there are, at present in use, two modes of adjusting these instruments, which are founded on different principles.

In the one, two points are taken on the limb of the circle; and when these are brought into a given position, by means of a plumb-line passing over them, the microscope or index is made to coincide with the zero point of the divisions: by this method, the error in collimation remains constant; and, if the adjustment is by any accident deranged, it can easily be rectified, and there will be no absolute necessity for frequently reversing the instrument; so that this method seems well adapted for large instruments, particularly if placed on stone piers. But it is liable to this defect, that the adjustment cannot be examined at the moment of observation; and if any change should take place in the general position of the frame work, the observation will be erroneous without the means of detection. It was probably to avoid this inconvenience, that

Mr. Troughton, in most of his instruments, particularly if they were intended to move freely in azimuth, has preferred the other method.

In this case, the plumb-line is attached to one of the pillars which support the microscopes in the way above described; and it has no reference to any fixed points or divisions on the limb of the circle, but only insures a similarity of position in the index, for each position of the instrument; and, provided that the plumb-line apparatus was free from all danger of derangement, this would be sufficient. This verification may be rendered perhaps more intelligible, by considering that a circular instrument, in whatever manner its vertical axis be placed, indicates by a double observation, the angle which the object makes with the axis, round which the whole instrument has revolved in passing from one position to the other. For let Pp be the axis, Tx the telescope in one position; it is evident, that in turning the instrument half round, ty will then be the position of the telescope: Px being equal to Py. The arc xy, which the telescope passes through to regain its former position, is the quantity really given by the instrument; and if the axis Pp be vertical, half this quantity is the true zenith distance of the object. Now the intention of Mr. Troughton's verification is to insure a vertical position to the axis Pp.



For instruments which rest on moveable pillars, and turn freely in azimuth, this method is much to be preferred; but it is not without a considerable defect: for, if by any derangement in the plumb-line apparatus, the error in collimation be changed, it cannot be restored with certainty to its former position; so that sometimes a very valuable series of observations

may be lost, for want of a corresponding one to compare with it. The mode which I propose to adopt to remedy these inconveniences, will enable us to combine all the advantages of the two methods above described: it is extremely simple in its principle, and easy of execution, for it merely consists in uniting on the same plumb-line the two principles already explained.

Two very fine holes should be made in the farther limb of the circle, and two lenses firmly fixed opposite to them, in the other, which should each form an optical image of its corresponding dot or hole, in the tube through which the plumb-line passes.* It will be best, if these dots are made exactly in a diameter, as they may then be used in two positions. Beneath these should be formed the image of a luminous point, according to Mr. Troughton's present method, by an apparatus attached to the plumb-line tube; when the two points on the circle move away, by the necessary operation in observing, the lower point will remain stationary, and indicate any change of position in the whole instrument, if such should accidentally take place, and which by the other method alone would have passed unnoticed.

The contrivance above described was executed for me at my request by Mr. Troughton, and is represented in the Plate; but by some accident a part of the apparatus was

* As these transparent dots are intended to be bisected by the plumb line, they must be capable of the necessary adjustments, both for distinct vision, and for placing them in an exact diameter.

It may be found more convenient in practice to arrange the whole apparatus in sliding tubes, but in whatever way the contrivance be executed, the points should ultimately be fixed as firmly as the divisions of the instrument.

broken in putting it together, so that I never was able to use it. As each apparatus for this adjustment is quite independent of the other, no possible inconvenience can attend their application, as either may be employed alone, at the option of the observer. But as any verification requiring many bisections is objectionable, I would in general certainly prefer Mr. TROUGHTON'S method, and only have recourse to the other, when there was reason to suspect that some alteration had taken place to render it necessary.

One more circumstance respecting the instrument remains to be noticed: when the divisions were first examined by opposite readings, 1″,25 was the greatest possible error which was to be apprehended, and 0″,7 the mean error; but in its journey it seemed to have suffered some very small derangement in its form: this was discernible both from examining the opposite readings; and by deducing the error of collimation by zenith stars, and comparing it with that found by an horizontal object, there was constantly perceived a difference of 3″ between the error of collimation deduced from γ Draconis and by an horizontal object; and this quantity was very uniformly distributed through the intermediate arc. In what particular manner the observations would be affected by this derangement I will not venture to decide, but I think it most likely that it has only rendered the instrument rather less accurate than it was originally, as is above stated. I have before observed the great advantage the circle possesses of showing the amount of its own errors. These may be determined with great certainty by examining the errors of collimation as deduced from different stars. This method is founded upon the supposition that half the difference of the two extreme

quantities is the greatest error of division, which has in this case influenced each result in an opposite direction. For instance, let us suppose the errors of division never to exceed 2″, but occasionally to amount to that quantity, on several parts of the circle; it will then sometimes occur that each index will give 2″ too much in one position of the instrument, and 2″ too little in the other; there will then appear a difference of 4″ in the error of collimation; but the observations in these extreme cases will not on that account be the less to be depended on; on the contrary, the probability is in favour of their superior accuracy.

Nor, on the other hand, will those observations which give the mean error of collimation deserve greater confidence than the rest, since it is evident that some of them may be, and most probably are, affected with the greatest possible error; for we suppose the most erroneous observation to arise from the greatest error of division occurring on each of the four arcs in the same sense, that is all *plus* or all *minus;* nevertheless, the observation thus erroneous, will give the mean error of collimation.

By an attentive consideration of these circumstances, corrections might perhaps be obtained which would somewhat diminish the probability of error. But it is to the principle of the revolving microscopes, that in the future construction of instruments we should look for perfection. In the French circle of repetition, too great a sacrifice is made to the supposed advantage of reading off a great number of observations at once. Our best instruments are too well constructed to stand in need of this contrivance, as the divisions on a two-feet circle are read off with precision to a single second.

The errors of simple division alone are those which continued observations have no tendency to diminish; these, by making the microscopes revolve, may be completely done away. An instrument thus constructed would be well adapted to detect small motions in the fixed stars which hitherto have escaped notice, or such as are but imperfectly known; for we cannot reasonably conclude that what is termed the proper motion of a star is so uniform and constant, that being once determined, it will remain always the same.

In the following observations, the larger number expresses the altitude, $+90°$, and the smaller number the zenith distance; the thermometer was attached to the telescope.

In reducing the observations of 1801, the proper motions are allowed for, according to the latest tables of Dr. MASKELYNE.

γ *Pegasi.*

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45 43,0.
1800	° ′ ″	Inches.	°	″	° ′ ″	° ′ ″
Nov. 3	142.50.27,5	29,2	48	42,5		
7	37. 9.27,8	29,2	51	42,5	37. 9.30,1	75.55.36,7
11	37. 9.26,9	29,6	48	43,3		
17	142.50.50	29,7	47	43,7	37. 9.28,4	75.55.35,0
18	37. 9.46,7	30,1	44	44,5		
19	37. 9.47	30,2	41	45	37. 9.31,3	75.55.38,0
20	142.50.44,5	30,3	41	45,1		
25	142.50.24,6	29,0	51	42,4	37. 9.31,7	75 55.38,3
26	37. 9.28	29,5	44	43,7		
27	142.50.26,8	29,6	42,5	43,9	37. 9.32,3	75.55.38,9
29	37. 9.31,5	30,0	42	44,5		
30	37. 9.28,2	30,0	45	44,2	37. 9.29,4	75.55.36,0
Dec. 6	142.50.29,5	29,2	40	43,7		
7	142.50.26,5	29,4	40	44,0	37. 9.30,2	75.55.37,0
8	37. 9.27	29,5	38	44,4		
16	37. 9. 2,1	30,0	44	44,3		
24	142.49.54,5	29,3	48	43,0	37. 9.30,1	75.55.36,0
25	37. 8.54,7	29,4	47	43,2		
31	37. 9. 0,2	29,9	39	44,7		
1801 Jan. 1	37. 9. 0	29,7	43	44,0	37. 9.32,3	75.55.37,5
3	142.49.55,9	29,7	47	43,6		
	Mean polar distance for Jan. 1800				- -	75.55.37,0

α *Arietis.*

1800 Dec. 24	151.16.12,7	29,4	46	31,3	28.43.12,9	67.29.22,0
25	28.42.38,5	29,4	46	31,3		
26	28.42.37,2	29,4	43	31,5		
27	28.42.38,2	29,6	43	31,7	28.43.11,7	67.29.20,8
1801 Jan. 1	28.42.38,2	29,7	45	31,7		
3	151.16.14,6	29,8	44	31,9		
7	28.43.33,0	30,0	45	32,0		
10	28.43.32,0	30,1	47	32,0		
14	28.43.30,0	29,9	45	31,9	28.43.11,3	67.29.20,0
16	151.17. 9,0	29,6	46	31,5		
17	151.17. 8,7	29,6	46	31,5		
	Mean polar distance for Jan. 1800				- - -	67.29.20,6

α Ceti.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co latitude 38.45.43,0
1801	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Jan. 14	47.56.29,6	29,9	45	1.4,4		
16	132. 4.13,2	29,7	45	1.3,8	47.56. 8,4	86.42. 9,4
17	132. 4.12,3	29,6	45	1.3,5		
24	132. 3.47,9	29,8	35	1.5,8	47.56.10,7	86.42.11,2
25	47.56. 9,3	29,8	35	1.5,8		
	Mean	-	-	-	-	86.42.10,3

Aldebaran.

1800						
Dec. 24	144.51. 8,2	29,4	46	40,2	35. 8.17,5	73.54.17,5
1801						
Jan. 1	35. 7.43,3	29,7	43	41,0		
20	144.51.43,2	29,9	48	40,8	35. 8.15,5	73.54.14,5
24	35. 8.14,3	29,8	35	41,8		
25	35. 8.14,8	29,8	35	41,8	35. 8.16,0	73.54.14,8
29	144.51.42,6	29,7	45	40,7		
Feb. 6	144.51.41,3	30,0	49	40,7	35. 8.17,0	73.54.15,5
7	35. 8.15,4	30,1	45	41,2		
	Mean polar distance Jan. 1800	-	-	-		73.54.15,5

Capella.

1800						
Dec. 24	174.32. 3,5	29,4	46	5,5	5.27.19,2	44.13.19,1
1801						
Jan. 1	5.26.42,1	29,7	43	5,6		
14	5.27.31,1	29,9	45	5,6	5.27.14,8	44.13.16,9
17	174.33. 1,5	29,6	45	5,5		
20	174.32.39,4	29,9	48	5,5	5.27.14,8	44.13.17,6
24	5.27. 9,0	29,8	35	5,8		
27	5.27. 9,7	29,7	41	5,6	5.27.14,9	44 13.18,3
29	174.32.39,9	29,7	46	5,6		
Feb. 23	5.27. 3,8	29,2	46	5,5	5 27.12,8	44.13.17,7
24	174.32.38,2	29,6	42	5,6		

Capella (continued.)

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan 1800. The Co-latitude 38.45.43,0.
	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
26	5.27. 3,6	29,9	45	5,6	5.27.12,6	44.13.17,7
Mar. 4	174.32.38,4	30,3	51	5,6		
7	174.32.37,9	30,2	51	5,6	5.27.14,0	44.13.19,8
8	174.32.37,7	30,0	46	5,6		
12	174.32.38,0	29,4	45	5,5		
23	5.27. 6,0	29,3	48	5,5		
31	5.27.27,5	30,2	55	5,5	5.27.14,9	44.13.19,5
April 1	174.32.57,7	30,2	59	5,5		
	Mean - - - - - - - -					44.13.18,5

Rigel.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan 1800.
1801						
Jan. 14	59.41. 6,0	29,9	45	1.39,2	59.40.45,3	98.26.36,5
17	120.19.35,5	29,6	45	1.38,2		
20	120.19 11,5	29,9	48	1.38,5	59.40.48,0	98.26 38,5
24	59 40 47,4	29,8	35	1.41,2		
27	59 40.46,0	29,7	41	1.39,5	59.40.47,5	98.26.37,5
29	120.19.11,2	29,7	46	1.38,3		
Feb. 6	120.19.13,7	30,0	50	1.38,3	59.40.46,0	98.26.35,0
7	59.40.45,5	30,1	46	1.39,5		
23	120.19. 7,4	29,2	42	1.37,6	59.40.49,5	98.26.37,5
26	59.40.46,5	29,9	45	1.39,0		
Mar. 3	120.19. 6,2	30,3	53	1.38,8	59.40.48,0	98.26.35,5
4	120.19. 7,7	30,3	51	1.39,3		
7	120.19. 3,5	30,2	46	1.39,0		
8	120.19. 6,5	30,0	45	1.38,6		
12	120.19. 5,0	29,4	48	1.37,0		
23	59 40 42,0	29,3	45	1.37,3		
31	59.41. 2,0	30,2	55	1.37,8	59.40.48,4	98.26.35,9
April 1	120.19.25,2	30,2	59	1.37,0		
2	59.41. 5,8	30,1	63	1.35,6	59.40.50,0	98.26.37,6
3	120.19.26,0	30,0	64	1.35,0		
	Mean - - - - - - - -					98.26.36,5

β *Tauri.*

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38 45.43,0.
1800	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Dec. 24	157.10.53,2	29,4	46	24,0	22.48.34,8	61.34.32,5
1801 Jan. 1	22.48. 2,9	29,7	43	24,4		
14	22.48.55,0	29,9	45	24,4	22.48.35,0	61.34.32,5
17	157.11.45,0	29,6	45	24,2		
20	157.11.21,2	29,9	48	24,3	22.48.36,6	61.34.34,0
24	22.48.34,5	29,8	35	25,0		
25	22,48.35,0	29,8	35	25,0	22.48.36,5	61.34 34,0
27	22.48.34,6	29,7	41	24,6		
29	157.11.21,8	29,7	46	24,2		
Feb. 6	157.11.22,8	30,0	50	24,2	22.48.35,1	61.34.33,0
7	22.48.33,0	30,1	46	24,5		
23	157.11.18,3	29,2	42	24,0	22.48.35,2	61.34.33,5
26	22.48.29,0	29,9	45	24,5		
Mar. 3	157.11.19,2	30,3	53	24,3	22.48.35,1	61.34.33,7
4	157.11.21,1	30,3	51	24,4		
7	157.11.17,5	30,2	46	24,6		
8	157.11.17,2	30,0	45	24,6		
12	157.11.19,3	29,4	48	23,9		
23	22.48.28,8	29,3	45	23,8		
31	22.48.50,6	30,2	55	24,1	22.48.35,9	61.34.34,0
April 1	157.11.38,8	30,2	59	23,9		
	Mean -	-	-	-	- -	61.34.33,7

α *Orionis.*

1801						
Jan. 14	43.52.59,5	29,9	45	56,0	43.52.38,8	82.38.30,5
17	136. 7.42,0	29,6	45	55,4		
20	136. 7.16,5	29,9	48	55,5	43.52.41,6	82.38.31,0
24	43.52.39,7	29,8	35	57,2		
27	43.52.39,2	29,7	41	56,2	43.52.40,5	82.38.30,5
29	136. 7.18,2	29,7	45	55,7		
Feb. 6	43.52.39,7	30,0	50	55,4	43.52 41,9	82,38.31,4
7	136. 7.16,0	30,1	46	56,2		
	Mean -	-	-	-	-	82.38.31

Sirius.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45.43,0.
1801	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Jan. 25	67.41.23,0	29,8	35	2.24,1	67.41.22,0	106.27. 1,8
29	112.18.39,2	29,8	44	2.22,0		
Feb. 20	67.41.24,0	29,5	44	2.20,5	67.41.26,0	106.27. 3,0
22	112.18.32,2	29,2	43	2.19,5		
27	112.18.31,5	29,9	45	2.21,0	67.41.25,5	106.27. 1,5
Mar. 4	67.41.22,5	30,3	49	2.21,3		
	Mean	-	-	-	- -	106.27. 2,1

Castor.

1801						
Feb. 26	18.55.22,0	29,9	45	19,5	18.55.30,2	57.41.14,8
27	18.55.21,8	29,9	45	19,5		
Mar. 4	161. 4 24,5	30,3	50	19,7		
7	161. 4 20,5	30,3	45	19,8		
8	161. 4 22,9	30,0	44	19,6	18.55.27,2	57.41.12,6
15	18.55 17,2	29,6	42	19,5		
April 2	18.55.41,3	30,1	56	19,1	18.55.27,1	57.41.13,5
3	161. 4.47,2	30,0	58	19,0		
17	18.55.47,3	29,8	50	19,3	18.55.29,1	57.41.15,5
18	161. 4.49,1	30,0	50	19,4		
	Mean	-	-	-	- -	57.41.14,0

Procyon.

1801						
Feb. 20	45.30.42,9	29,5	45	58,4	45.30.45,7	84.16.22,1
22	134.29.11,4	29,2	43	58,1		
24	45.30.37,8	29,6	40	59,3	45.30.45,1	84.16.21,2
26	45.30 44,2	29,9	45	59,2		
27	45.30.41,2	29,9	45	59,2		
Mar. 4	134.29.12,2	30,3	50	59,3		
7	134.29 10,0	30,3	45	60,0		
8	134.29.10,0	30,0	44	59,5		
15	45.30 38,5	29,6	42	59,0		

Procyon. (continued.)

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45.43,0.
1801	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
April 2	45.31. 1,0	30,1	56	58,0	45.30.45,0	84.16.21,1
3	134.29.31,0	30,0	58	57,5		
17	45.31. 7,8	29,8	50	58,3	45 30.46,0	84 16.21,6
18	134.29.35,8	30,0	50	58,7		
19	134.29.38,0	30,0	52	58,5		
	Mean	-	-	-	-	84.16.21,5

Pollux.

1801						
Feb. 24	22.44.24,5	29,6	40	24,5	22.44.31,5	61.30.14,0
26	22.44.27,4	29,9	45	24,7		
27	22.44.27,0	29,9	45	24,7		
Mar. 4	137.15.21,1	30,3	50	24,4		
7	137.15.22,7	30,3	45	24,8		
8	137.15.23,1	30,0	44	24,6		
15	22.44.23,0	29,6	42	24,3		
April 2	22.44.44,5	30,1	56	24,0	22.44.29,2	61.30.12,7
3	157.15.46,2	30,0	58	23,8		
	Mean	-	-	-	-	61.30.13,7

Regulus.

1800						
Oct. 31	38.17.54,6	30,0	45	46,12	38.17.57,3	77. 3.34,0
Nov. 5	141.42.00	29,5	44	45,48		

β Leonis.

1800						
Nov. 13	35.32.59,9	29,8	50	41,7	35.33. 2,6	74.18.31,5
14	144.26.54,6	29,4	50	41,7		
17	35.33.15,2	29,9	43	42,5	35.33. 1,8	74.18.29,5
18	144.27.11,7	30,1	38	42,8		
1801						
Nov. 8	144.27.22,0	30,0	42	42,0	35.33.27,0	74.18.35,5
13	35.33.15,8	30,0	44	41,8		
	Mean	-	-	-	-	74.18.32,5

Spica Virginis.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.15.43,0.
1800	° ′ ″	Inches	°	′ ″	° ′ ″	° ′ ″
Nov. 17	61.21.31,5	30,0	45	1.46,5		
18	61.21.29,7	30,1	40	1. 4,8	61.21.11,3	100. 6.43,3
19	118.39. 8,0	30,3	40	1.48,5		
25	118.38.48,6	29,3	44	1.44,5		
27	118.38.45,8	29,7	39	1.47,2	61.21.12,3	100. 6.42,9
Dec. 1	61.21.13,3	29,6	44	1.45,2		
2	61.21.10,2	29,4	43	1.45,0		
7	118.38.44,5	29,3	38	1.46,0		
9	61.21.12,1	29,6	38	1.46,8	61.21.13,3	100. 6.42,5
10	61.21.11,3	29,6	41	1.46,1		
12	61.21.10,2	29,8	42	1.46,7		
	Mean	-	-	- -	-	100. 6.43,0

Arcturus.

1800						
Nov. 16	148.59.34,1	29,6	48	34,3	31.00.41,8	69.46. 8,5
17	31.00.57,6	30,0	46	35,1		
18	31.00.55,8	30,2	42	35,6	31.00.42,3	69.46. 8,2
19	148.59.31,1	30,3	42	34,6		
25	148.59. 7,9	29,3	45	35,0	31.00.46,6	69.46.10,7
26	31.00.41,2	29,6	41	35,0		
27	148.59. 8,5	29,8	39	35,2	31.00.46,5	69.46.10,5
28	31.00.41,6	29,8	42	35,2		
Dec. 1	31.00.42,7	29,6	44	34,7	31.00.45,0	69.46. 8,0
2	148.59.12,7	29,4	44	34,5		
6	148.59. 8,2	29,3	38	35,0		
7	31.00.40,8	29,6	40	35,0	31.00.46,7	69.46. 8,5
9	31.00.42,4	29,6	38	35,1		
	Mean -	-	-	-	- -	69.46. 9,0
Corrected for the proper motion of the star					- -	69.46. 7,5

α Coronæ.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45.43,0.
1800	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Nov. 5	23.50.37,0	29,6	49	25,2		
6	23.50.39,4	29,3	51	24,9	23.50.42,2	62.36.13,0
13	156. 9.13,9	29,7	48	25,4		
17	156. 9.34,8	29,6	49	25,2		
19	23.50.59,0	30,2	44	26,0	23.50.42,6	62.36.11,5
20	156. 9.32,8	30,3	43,5	26,1		
25	156. 9. 9,7	29,4	46	25,3	23.50.46,5	62.36.13,5
26	23.50.41,8	29,6	42	25,7		
27	156. 9. 7,6	29,8	41	25,9	23.50.47,7	62.36.14,4
28	23.50.43,1	29,8	44	25,7		
29	23.50.43,7	30,0	44	25,8	23.50.47,9	62.36.13,4
Dec. 2	156. 9. 7,9	29,4	44	25,4		
7	23.50.44,4	29,5	41	25,7	23.50.49,1	62.36.13,0
9	23.50.44,9	29,6	39	25,7		
26	23.50.23,3	29,6	41	25,6		
30	23.50.24,0	29,5	35	25,8	23.50.56,6	62.36.14,0
1801 Jan. 3	156. 8.30,4	29,7	44	25,6		
	Mean	-	-	- -	-	62.36.13,0

α Serpentis.

1800						
Dec. 2	135.49.25,0	29,4	24,4	55,8		
7	44.10.34,5	29,5	40	56,5	44.10.34,0	82.56. 2,0
9	44.10.34,3	29,6	39	56,8		

α Ophiuchi.

1800						
Nov. 5	38.31.10,4	29,6	49	45,6		
13	141.28.44,3	29,7	48	45,7	38.31.13,5	77.16.52,2
14	141.28.42,6	29,8	51	45,6		
19	38.31.31,4	30,2	44	46,9	38.31.21,1	77.16.54,5
20	141.28.48,9	30,3	44	47,0		

α *Ophiuchi.* (*continued.*)

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45.43,0.
1800	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Nov. 26	38.31.13,2	29,4	46	45,4	38.31.16,7	77.16.53,3
27	141.29.39,9	29,6	45	45,8		
Dec. 25	38.30.51,6	29,4	44	45,6	38.31.24,0	77.16.53,6
26	38.30.51,5	29,6	43	46,0		
1801						
Jan. 3	141.28. 3,3	29,7	46	45,9		
	Mean	-	-	-	-	77.16.53,5

α *Lyræ.*

1800						
Oct. 31	12.37.42,6	29,9	50	12,7	12.37.46,5	51.23.36,5
Nov. 3	167.22. 9,6	29,2	51	12,6		
17	167.22.28,2	29,6	49	12,8	12.37.46,8	51.23.34,2
18	12.38. 0,8	30,0	45	13,0		
20	167.22.26,1	30,3	43	13,2		
25	167.22. 2,7	29,0	56	12,3	12.37.51,0	51.23.36,7
26	12.37.44,5	29,4	46	12,8		
28	167.22. 4,5	29,8	41	13,1	12.37.48,7	51.23.33,8
29	12.37.42,0	29,9	43	13,1		
30	12.37.45,0	30,0	46	13,0	12.37.52,6	51.23.36,8
Dec. 2	12.37.47,9	29,6	44	12,9		
3	167.22. 2,0	29,4	45	12,8		
8	167.22. 0,1	29,5	42	12,9		
24	167.21.25,7	29,3	50	12,6	12.37.59,0	51.23.35,0
25	12.37.23,7	29,4	48	12,7		
1801						
Jan. 2	167.21.26,4	29,7	50	12,8		
3	167.21.23,3	29,7	46	12,9		
14	12.38.22,7	29,9	48	13,0	12.38. 1,8	51.23.34,0
15	167.22.20,0	29,7	48	13,0		
17	167.22.18,3	29,6	45	12,9		
21	12.38. 3,0	29,6	48	12,8	12.38. 8,0	51.23.37,8
22	12.38. 3,0	29,4	40	13,0		
23	167.21.48,0	29,6	36	13,2		
24	167.21.46,0	29,6	36	13,2		

α Lyræ. (continued.)

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45.43,0.
1801	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Feb. 4	167.21.45,7	29,9	51	12,8		
5	167.21.43,7	29,8	50	12,8		
6	167 21.44,0	3[illegible],1	45	13,0	12.38.11,8	51.23.37,4
9	12.38. 6,1	30,1	40	13,1		
13	12.38. 8,0	29,6	31	13,3		
22	167.21.38,1	29,2	40	13,0	12.38.13,0	51.23.35,8
23	12.38. 4,0	29,3	40	13,3		
Nov. 9	12.37.32,4	30,0	50	12,9		
10	12.37.32,3	29,0	51	12,8	12.37.43,6	51.23.35,5
14	167.22. 5,1	30,0	49	12,9		
21	12.37.32,5	29,4	45	12,8		
30	12.37.34,2	28 9	40	12,7		
Dec. 10	12.37.37,7	29,3	46	12,7	12.37.49,3	51.23.35,3
13	167.21.56,5	29.6	46	13,0		
14	167.21.56,0	29,6	38	13,0		
	Mean	-	-	-	-	51.23.36,0

α Aquilæ.

1800						
Nov. 18	42.53.14,0	30,0	45	54,0	42.53.00,0	81.38.49,0
20	137. 7.13,9	30,3	43	54,7		
26	42.53. 1,2	29,4	44	53,4	42.53. 3,9	81.38.52,0
28	137. 6.53,3	29,6	42	53,9		
29	42.52.57,5	29,9	44	54,2	42.53. 2,6	81.38.50,2
Dec. 3	137. 6.52,3	29,4	45	53,1		
24	137. 6.17,9	29,3	48	52,7	42.53. 8,5	81.38.52,7
31	42.52.35,0	29,7	39	54,5		
1801						
Jan. 3	137. 6.19,6	29,7	50	52,9		
4	137. 6.19,2	29 7	48	53,1		
14	42.53.29,1	29,9	48	53,6	42.53. 9,5	81.38.50,7
15	137. 7.10,0	29,7	45	56,6		
22	42.53.10,8	29,4	40	53,8	42.53.14,0	81.38.53,0
Feb. 6	137. 6.42,7	31,0	45	54,3		
	Mean	-	-	-	-	81.38.51,5

α ♑.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan 1800. The Co latitude 38.45.43,0.
1801	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Nov. 18	64.23.39,0	30,0	45	2. 1,5		
19	64.23.40,8	30,1	44	2. 2,0	64.23.24,5	103. 9. 8,6
20	115.36.51,8	30,3	42,5	2. 3,2		
26	64.23.19,2	29,5	45	1.59,2	64.23.22,5	103. 9. 6,6
28	115.36.34,0	29,8	42	2. 1,4		
	Mean	-	-	-	-	103. 9. 7,6

α Cygni.

1800						
Nov. 17	173.20.38,8	29,6	49	6,7	6.39.32,2	45.25.34,1
18	6.39.43,3	30,0	45	6,8		
19	6.39.46,1	30,2	44	6,8	6.39.33,7	45.25.35,3
20	173.20.38,6	30,3	42	6,9		
25	173.20.16,0	29,0	53	6,5	6.39.37,2	45.25.37,8
26	6.39.30,5	29,4	45	6,7		
28	173.20.18,4	29,8	41	6,8	6.39.35,0	45.25.35,5
29	6.39.28,3	29,9	43	6,8		
30	6.39 29,4	30,0	46	6,8	6.39.35,8	45.25.36,0
Dec. 2	173.20.17,8	29,6	44	6,7		
3	173.20.17,8	29,4	44	6,7	6.39.35,0	45.25.35,0
8	6.39.28,3	29,5	42	6,8		
16	6.39. 5,0	29,6	38	6,8		
25	6.39. 5,2	29,4	48	6,7		
31	6.39. 6,3	29,7	48	6,7	6.39.43,7	45.25.36,8
1801						
Jan. 2	173.19.39,7	29,9	43	6,8		
3	173.19.39,8	29,7	50	6,7		
22	6.39.47,4	29,6	45	6,7		
23	6.39.47,0	29,4	40	6,8	6.39.52,4	45.25.39,5
25	173.20. 2,6	29,6	36	6,9		
29	173.20. 2,0	29,8	46	6,7		
Feb. 19	6.39.54,2	29,5	46	6,7	6.39.59,9	45.25.40,2
22	173.19.54,3	29,2	42	6,7		
April 2	6.40.17,2	30,0	54	6,7	6.40. 5,2	45.25.39,0
4	173.20. 6,8	29,8	43	6,7		

α *Cygni. (continued.)*

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan 1800. The Co-latitude 38.45.43,0.
1801	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
April 5	6.40.20,7	29,8	45	6,7	6.40. 6,0	45.25.39,7
7	173.20. 8,8	29,6	45	6,7		
8	173.20.10,2	29,6	40	6,8	6.40. 4,1	45.25.37,7
10	6.40.18,3	29,5	42	6,8		
11	6.40.22,8	29,6	40	6,8	6.40. 6,3	45.25.39,7
12	6.40.22,5	30,2	35	7,0		
13	173.20. 9,6	30,2	39	6,9		
14	173.20.10,7	30,0	42	6,8		
Nov. 8	173.20.27,2	29,8	45	6,8	6.39.22,3	45.25.37,6
9	6.39.11,8	30,0	48	6,8		
13	6.39.10,0	29,9	48	6,8	6.39.21,3	45.25.36,3
14	173.20.27,4	30,0	48	6,8		
18	173.20.24,2	29,5	43	6,7	6.39.22,3	45.25.36,6
19	6.39. 8,8	29,6	42	6,7		
26	173.20.22,5	29,4	45	6,7	6.39.22,7	45.25.36,0
30	6.39. 9,7	29,0	40	6,7		
Dec. 1	6.39. 9,7	29,0	40	6,7	6.39.23,0	45.25.35,8
2	6.39. 9,2	29,0	40	6,7		
3	173.20.23,2	29,6	40	6,8		
7	173.20.24,7	29,4	40	6,8		
9	173.20.23,2	28,7	47	6,5	6.39.24,0	45.25.35,6
11	6.39.10,9	29,6	45	6,7		
13	173.20.21,2	29,6	38	6,8	6.39.26,0	45.25.36,2
14	173.20.22,7	29,6	38	6,8		
16	6.39.14,5	29,3	35	6,8		
17	6.39.14,0	29,5	35	6,8		
	Mean	-	-	-	-	45.25.37,0

α Aquarii.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45.43,0.
1800	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Nov. 17	137.29. 6,7	29,7	46	1.14,8	52.31. 8,7	91.17. 4,3
18	52 31.24,1	30,1	45	1.16,3		
19	52.31.26,7	30,2	41	1.17,2	52.31.11,0	91.17. 6,4
20	137.29. 4,8	30,3	41	1.17,5		
26	52.31. 3,9	29,5	44	1.14,9	52.31. 7,7	91.17. 2,7
28	137.28.48,4	29,8	41	1.16,3		
29	52.31. 6,1	29,9	43	1.16,1	52.31.10,2	91.17. 5,0
Dec. 2	137.28.45,7	29,5	44	1.15,0		
3	137.28.47,2	29,4	42	1.15,0	52.31.10,0	91.17. 4,6
8	52.31. 7,3	29,5	40	1.15,7		
	Mean	-	-	-	- -	91.17. 4,6

α Pegasi.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800.
1800						
Nov. 17	142.54.25,9	29,7	46	43,5	37. 5.53,0	75.51.57,2
18	37. 6.11,9	30,1	44	44,4		
19	37. 6.10,0	30,2	40	45,0	37. 5.54,1	75.51.58,0
20	142.54.21,8	30,3	40	45,2		
26	37. 5.49,8	29,5	44	43,5	37. 5.54,8	75.51.58,7
27	142.54.00,1	29,6	42	43,9		
Dec. 24	142.53.30,0	29,3	47	43,0	37. 5.56,8	75.51.58,7
25	37. 5.23,7	29,4	47	43,0		
26	37. 5.25,0	29,4	43	43,4	37. 5.56,0	75.51.57,2
31	37. 5.24,0	29,8	38	44,4		
Jan. 1	37. 5.22,8	29,7	45	43,6		
3	142.53.32,1	29,7	47	43,4		
	Mean	-	-	-	- -	75.51.58,0

α Andromedæ.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38.45.43,0.
1800	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Nov. 17	156.45.37,2	29,75	47	24,8	23.14.38,4	62.00.48,4
18	23.14.54,0	30, 1	44	25,2		
19	23.14.58,2	30, 2	41	25,5	23.14.40,6	62.00.49,6
20	156.45.37,0	30, 3	41	25,5		
25	156.45.13,7	29, 0	51	24,0	23.14.41,0	62.00.51,2
26	23.14.35,8	29, 5	44	24,8		
27	156.45.14,2	29, 6	42,5	25,0	23.14.40,3	62.00.50,7
29	23.14.35,5	30, 0	42	25,3		
30	23.14.38,3	30, 0	45	25,0	23.14.39,0	62.00.49,4
Dec. 6	156.45.20,2	29, 2	40	24,8		
7	156.45.18,5	29, 4	39	25,0	23.14.38,3	62.00.48,5
8	23.14.35,0	29, 5	38	25,1		
16	23.14.11,5	30, 0	44	25,0		
24	156.44.45,1	29, 3	48	24,4	23.14.41,7	62.00.51,4
25	23.14. 5,5	29, 4	47	24,5		
31	23.14. 8,3	29, 9	39	25,3		
1801 Jan. 1	23.14. 8,5	29, 7	43	24,9	23.14.40,7	62.00.50,0
3	156.44.47,0	29, 7	47	24,7		
18	156.45.14,6	29, 9	46	25,1	23.14.44,0	62.00.50,7
22	23.14.42,5	29, 6	44	24,9		
	Mean	-	-	-	-	62.00.50,0

Polaris.

Day of the Month.	Zenith Distance corrected for Refraction.	Reduced to January, 1800.
1800	° ′ ″	° ′ ″
Nov. 2	37.00.30,3	37.00.3,2
4	37.00.30,8	37.00.3,0
7	37.00.31,3	37.00.2,4
11	37.00.32,7	37.00.2,7
17	37.00.36,0	37.00.4,2
18	37.00.36,3	37.00.4,3
19	37.00.35,7	37.00.3,4
20	37.00.36,3	37.00.3,8
21	37.00.37,6	37.00.4,8
25	37.00.40,1	37.00.5,8
27	37.00.39,5	37.00.4,9
29	37.00.40,3	37.00.5,2
30	37.00.40,1	37.00.4,8
Dec. 23	37.00.44,2	37.00.5,2
1801		
Jan. 1	37.00.44,3	37.00.4,6
21	37.00.44,2	37.00.5,0
24	37.00.44,7	37.00.5,8
25	37.00.44,3	37.00.5,2
Feb. 6	37.00.43,6	37.00.5,7
25	37.00 40,8	37.00.6,2
Mar. 2	37.00.39,3	37.00.5,8
25	37.00.33,4	37.00.5,6
31	37.00.30,6	37.00.4,2
April 1	37.00.30,9	37.00.4,8
2	37.00.29,7	37.00.3,9
3	37.00.29,9	37.00.4,5
4	37.00.29,2	37.00.4,4
5	37.00.28,4	37.00.3,9
13	37.00.28,2	37.00.5,5
June 11	37.00.15,0	37.00.3,5

N. B. These observations of the polar star were all made by reversing the instrument several times, before and after the star's passage over the meridian, allowing for the small change in altitude, according to the French tables constructed for that purpose.

Polaris. (continued.)

Day of the Month.	Zenith Distance corrected for Refraction.	Reduced to January, 1800.
1801	° ′ ″	° ′ ″
June 13	37.00.16,5	37.00.5,0
21	37.00.17,0	37.00.5,4
Nov. 26	37. 1. 2,0	37.00.6,0
Dec. 7, 12	37. 1. 5,5	37.00.6,0
13, 14	37. 1. 6,2	37.00.6,5
16, 17, 18, 19	37. 1. 5,5	37.00.5,2

Polaris, S. P.

1800		
Oct. 31	40.30.52,0	40.31.18,6
Nov. 3	40.30.54,0	40.31.18,1
4	40.30.51,2	40.31.19,2
13	40.30.48,0	40.31.18,8
17	40.30.46,8	40.31.18,8
18	40.30.45,9	40.31.18,1
19	40.30.46,5	40.31.19,0
26	40.30.44,6	40.31.19,1
27	40.30.43,2	40.31.18,5
28	40.30.44,0	40.31.19,0
Dec. 10	40.30.41,8	40.31.19,0
1801		
June 3	40.31. 4,0	40.31.16,5

Polaris, S. P. (continued.)

Day of the Month.	Zenith Distance corrected for Refraction.	Reduced to January, 1800.
1801	° ′ ″	° ′ ″
June 4	40.31. 5,7	40.31.17,6
5	40.31. 3,6	40.31.15,6
6	40.31. 5,7	40.31.17,5
8	40.31. 4,0	40.31.15,8
9	40.31. 3,5	40.31.15,2
10	40.31. 6,5	40.31.18,1
Nov. 8	40.30.27 7	40.31.18,7
13	40.30.25,8	40.31.18,2
18	40.30.26,0	40.31.20,0
19	40.30.25,9	40.31.20,1
20	40.30.25,1	40.31.19.5
25	40.30.23,2	40.31.19,0
27	40.30.23,4	40.31.19 8
Dec. 2	40.30.22.7	40.31.20,1
5	40.30.21,9	40.31.20,0

β Ursæ.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm	Refraction.	Zenith Distance corrected for Refraction.	Reduced to January, 1800.
1800	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Dec. 24	23.42.55,2	29,4	44	25,2		
25	156.15.52,4	29,4	43	25,2		
26	156.15.52,5	29,6	41	25,5	23.43.31,4	23.44. 6,0
1801						
Jan. 1	23.42.55,7	29,9	43	25,7		
July 20	23.43.48,7	30,0	72	25,0	23.43.57,8	23.44. 8,3
21	156.15.53,1	30,0	70	25,0		

β Ursæ. (continued.)

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Reduced to January, 1800.
1801	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Dec. 2	23.43.12,0	29,4	40	25,4		
6	23.43.11,9	29,4	40	25,4		
7	23.43.10,1	29,4	40	25,4	23.43.22,9	23.44. 8,8
9	156.16.27,1	29,3	46	25,0		
11	156.16.26,7	29,6	38	25,8		
12	23.43. 7,5	29,6	38	25,8		
13	23.43. 8,5	29,6	34	26,0	23.43.19,7	23.44. 8,2
18	156.16.28,5	30,0	32	26,5		
	Mean	-	-	-	- -	23.44. 7,8

β Ursæ, S. P.

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Reduced to January, 1800.
1800						
Dec. 15	126.11.38,6	30, 0	47	1.19,4		
16	126.11.35,7	30, 0	44	1.20,0		
24	53.47.22,1	29, 4	47	1.17,6	53.47.54,6	53.47.21,3
25	126.11.29,7	29, 4	46	1.17,8		
26	126.11.30,7	—	43	1.18,3		
27	126.11.30,2	29,67	43	1. 9,0		
1801						
Dec. 13	53.48. 1,6	29, 6	35	1.20,4	53.48. 9,5	53.47.21,5
14	126.11.42,5	29, 8	35	1.21,0		
16	126.11.38,8	29, 3	35	1.19,7		
17	126.11.36,4	29, 4	37	1.19,6	53.48.11,5	53.47.21,8
18	53.48. 0,3	29, 7	34	1.20,8		
19	53.48. 1,0	30, 1	33	1.23,0		
	Mean	-	-	-	- -	53.47.21,5

γ *Draconis.*

Day of the Month.	Observations corrected for Refraction.	Barom.	Therm.	Refraction.	Zenith Distance corrected for Refraction.	Mean Polar Distance for Jan. 1800. The Co-latitude 38°.45′.43″,0
1800	° ′ ″	Inches.	°	′ ″	° ′ ″	° ′ ″
Dec. 3	0.16.40,25			0,25		
7	0.16.38,25				0.16.44,25	38.28.53,7
8	179.43.10,75					
24	0.16. 1,75					
26	179.42.45, 6				0.16.37, 6	38.28.52,8
1801						
Jan. 3	0.15.59,75					
13	179.43.46,25				0.16.31, 1	38.28.52,7
17	0.16.48, 5					
22	179.43.29, 5					
23	0.16.23, 8				0.16.27, 2	38.28.54,6
24	179.43.29, 5					
Feb. 6	0.16.18, 7				0.16.23, 0	38.58.54,7
13	179.43.32, 7					
	Mean	-	-	-	-	38.28.53,8

After the first part of this Paper went to the press, Dr. Maskelyne communicated to me some late corrections which Mr. Piazzi has made to his Catalogue. These having been adopted in the Tables which follow, the positive deviations do not now exactly equal the negative; but the correction required is very small. The Greenwich, Palermo, and Westbury observations were made about the same period, between 1800 and 1802, those of Armagh in the year 1797.

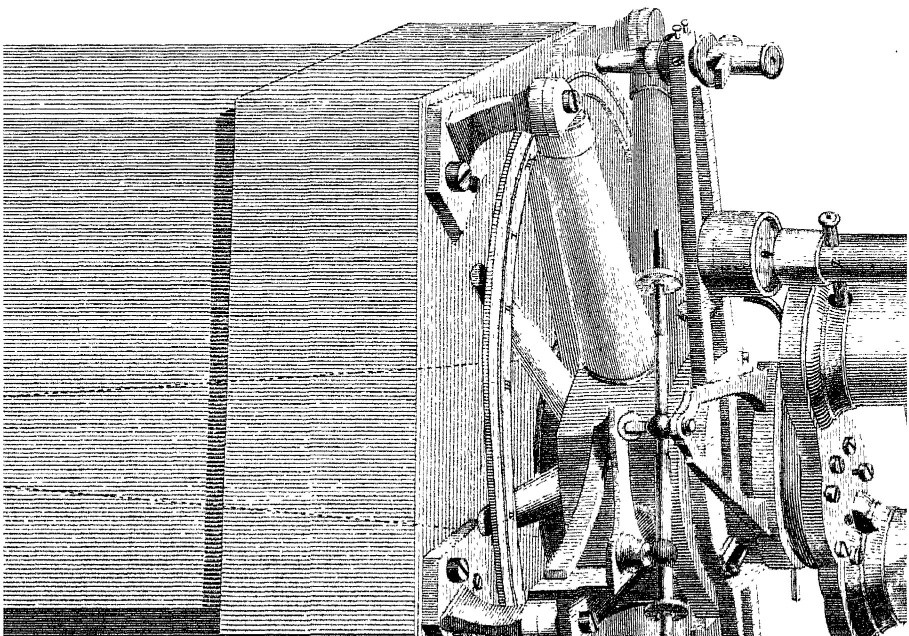
A Comparison of the Observations made at Greenwich, Armagh, Palermo, and Westbury, with a Catalogue deduced from the Mean of these, and of some other Observations made with different circular Instruments.

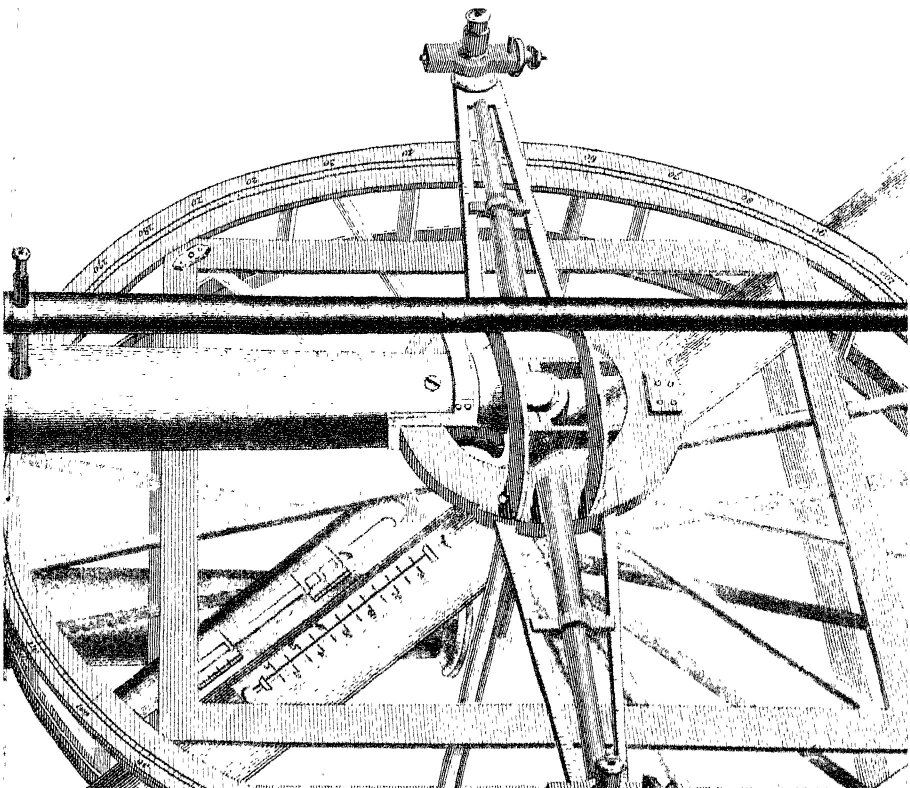
	Greenwich. January, 1800.		Armagh.		Palermo.		Westbury. Co-lat. 38°.45′.43″,0.		Mean of all the Observations, reduced to Jan. 1800.
	° ′ ″		° ′ ″		° ′ ″		° ′ ″		° ′ ″
γ Draconis -	38.28.53,0	−0,8	38.28.52,5	−1,3	38.28.53,0	−0,8	38.28.53,8	+0,0	38.28.53,8
Capella -	44.13.21,5	+1,5	44.13.20,0	0.0	44.13.18,5	−1,5	44.13.18,5	−1,5	44.13.20,0
α Cygni - -	45.25.41,4	+2,6	45.25.38,0	−0,8	45.25.39,6	−0,8	45.25.37,0	−1,8	45.25.38,8
α Lyræ - -	51.23.41,1	+3,4	51.23.35,8	−1,9	51.23.37,8	+0,1	51.23.36,0	−1,7	51.23.37,7
Castor - -	57.41.14,0	0 0	*57.41. 8,0	—	57.41.14,0	0 0	57.41.14,0	0.0	57.41.14,0
Pollux -	61.30. 9,8	−2,2	*61.30. 3,8	—	61.30.12,2	+0,2	61.30.13,7	+1,7	61.30.12,0
β Tauri -	61.34.30,9	−1,6	61.34.31,0	−1,5	61.34.34,8	+2,3	61.34.33,7	+1,2	61.34.32,5
α Andromedæ	62.00.45,8	−1,2	62.00.43,7	−3,3	62.00.48,5	+1,5	62.00.50,0	+3,0	62.00.47,0
α Coronæ Bor.	62.36.10,5	+0,3	*62.36. 6,0	—	62.36.10,8	+0,6	62.36.13,0	+2,8	62.36.10,2
α Arietis -	67.29.20,1	−1,3	67.29.22,0	+0,6	67.29.22,7	+1,3	67.29.20,6	−0,8	67.29.21,4
Arcturus -	69.46. 7,8	−1,2	69.46. 9,7	+0,7	69.46.11,0	+2,0	69.46. 7,5	−1,5	69.46. 9,0
Aldebaran	73.54.16,6	−0,2	73.54.17,0	+0,2	73.54.18,0	+1,2	73.54.15,5	−1,3	73.54.16,8
β Leonis -	74.18.34,5	+0,8	74.18.32,7	−1,0	74 18.35,0	+1,3	74.18.32,5	−1,2	74.18.33,7
α Pegasi -	75.51.57,0	−2,0	75.51.59,8	+0,8	75.52. 1,0	+2,0	75.51.58,0	−1,0	75.51.59,0
γ Pegasi -	75.55.36,3	−0,7	75.55.36,2	−0,8	75.55.38,5	+1,5	75.55.37,0	0.0	75.55.37,0
Regulus - -	77. 3.35,1	+1,1	77. 3.30,7	−3,3	77. 3.37,5	+3,5	77. 3.34,0	0.0	77. 3.34,0
α Ophiuchi	77.16.54,0	+0,6	77.16.52,1	−1,3	77.16.54,0	+0,6	77.16.53,5	+0,1	77.16.53,4
α Aquilæ -	81.38.52,0	+1,5	81.38.49,3	−1,1	81.38.55,0	+4,5	81.38.51,5	+1,0	81.38.50,5
α Orionis -	82.38.30,8	−0,7	82.38.30,5	−1,2	82.38.33,0	+2,3	82.38.31,5	0.0	82.38.31,5
α Serpentis -	82.56. 1,2	−0,8	82.55.58,5	−3,5	82.56. 5,8	+3,8	82.56. 2,2	+0,2	82.56. 2,0
Procyon -	84.16.17,4	−3,1	84.16.18,0	−2,5	84.16.22,0	+1,0	84.16.21,5	+1,0	84.16.20,5
α Ceti - -	86.42. 6,1	−1,5	86.42. 6,0	−2,2	86.42.10,8	+0,6	86.42.10,2	+2,0	86.42. 8,2
α Aquarii -	91.16.59,8	−3,2	91.17. 3,3	+0,3	91.17. 3,7	+0,7	91.17. 4,6	+1,6	91.17. 3,0
α Hydræ -	97.47.49,1	−1,5	97.47.47,5	−3,1	97.47.54,0	+3,4	97.47.53,0	+2,4	97.47.50,6
Rigel - -	98.26.28,8	−5,3	98.26.31,7	−2,4	98.26.35,5	+1,4	98.26.36,5	+2,5	98.26.34,1
Spica Virginis	100. 6.37,0	−3,0	100. 6.37,5	−2,5	100. 6.42,8	+2,8	100. 6.43,0	+3,0	100. 6.40,0
α Capricorni	103. 9. 3,2	−4,9	103. 9.12,0	+3,9	103. 9. 9,2	+1,1	103. 9. 8,0	−0,1	103. 9. 8,1
Sirius - -	106.26.56,3	−5,5	106.27. 3,8	+2,0	106.27. 5,0	+3,2	106.27. 2,0	+0,2	106.27. 1,8
Polaris - -	1.45.34,5	—	1.45.34,5	—	1.45.36,2	—	1.45.36,9	—	1.45.36,0

The observations marked * are omitted in the calculation.

The Greenwich Observations compared with those made by the circular Instruments, the Co-latitudes of the Places of Observation being previously corrected by Means of their positive and negative Deviations.

	Greenwich.		Armagh.		Palermo.		Westbury. Co-lat. 38°.45′.42″,8.		Mean of circular Instruments reduced to January, 1800.
	° ′ ″		° ′ ″		° ′ ″		° ′ ″		° ′ ″
γ Draconis -	38.28.53,0	−0,6	38.28.53,8	+0,2	38.28.52,0	−1,6	38.28.53,6	0.0	38.28.53,6
Capella - -	44.13.21,5	+1,5	44.13.21,3	+1,3	44.13.18,5	−1,5	44.13.18,3	−1,7	44.13.20,0
α Cygni -	45.25.41,4	+2,6	45.25.39,3	+0,5	45.25.38,4	−0,4	45.25.36,8	−2,0	45.25.38,8
α Lyræ - -	57.23.41,1	+4,6	51.23.37,1	+0,6	51.23.36,8	+0,3	51.23.35,8	−0,7	51.23.36,5
Castor - -	57.41.14,0	+1,0	*57.41. 9,3	—	57 41.13,0	0.0	57.41.13,8	+0,8	57.41.13,0
Pollux -	61.30. 9,8	−2,5	*61.30. 4,1	—	61.30.11,2	−1,1	61.30.13,5	+1,2	61.30.12,3
β Tauri -	61.34.30,9	−2,1	61 34.32,3	−0,7	61.34.33,8	−0,6	61.34.33,5	+0,5	61.34 33,0
α Andromedæ	62 00.45,8	−1,7	62.00.45,0	−2,5	62.00 47,5	0.0	62.00.49,8	+2,3	62.00.47,5
α Coron. B.	62.36.10,5	+0,5	62.36. 7,3	−2,7	62.36. 9,8	−0,2	62.36.12,8	+2,8	62.36.10,0
α Arietis -	67.29.20,1	−1,4	67.29.22,0	+0,5	67.29.21,7	+0,2	67.29.20,5	−1,0	67.29.21,5
Arcturus -	69.46. 7,8	−1,2	69.46.11,0	+2,0	69.46.10,3	+1,2	69.46. 7,3	−1,7	69.46. 9,0
Aldebaran	73.54.16,6	−0,3	73.54.18,3	+1,4	73.54.16,8	+0,1	73.54.15,3	−1,6	73.54.16,9
β Leonis -	74.18.34,5	+1,0	74.18.34,0	+0,5	74.18.33,5	+0,5	74.18.32,3	−1,2	74.18.33,5
α Pegasi -	75.51.57,0	−2,7	75.52. 1,0	+1,3	75.52. 0,8	+1,1	75.51.57,8	− ,9	75.51.59,7
γ Pegasi -	75.55.36,3	−1,9	75.55.37,5	−0,7	75.55.41,4	+3,2	75.55.36,8	−1,4	75.55.38,2
Regulus -	77. 3.35,1	+1,1	77. 3.32,0	−2,0	77. 3.36,0	+2,0	77. 3.33,8	−0,2	77. 3.34,0
α Ophiuchi	77.16.54,0	+0,8	77.16.53,4	+0,2	77.16.53,0	−0,2	77.16.53,3	+0,1	77.16 53,2
α Aquilæ -	81.38.52,0	+1,0	81.38.50,6	−0,4	81.38.53,5	+2,5	81.38.51,3	+0,3	81.38.51,0
α Orionis -	82.38.30,8	−0,9	82.38.31,8	+0,1	82.38.32,0	+0,3	82.38.31,3	−0,4	82.38.31,7
α Serpentis	82.56. 1,2	−1,1	82.55.59,8	−2,4	82.56. 5,2	+2,9	82.56. 2,0	−0,3	82.56. 2,3
Procyon -	84.16.17,4	−3,6	84.16.19,3	−1,7	84.16.21,0	0.0	84.16.21,3	+0,3	84.16.21,0
α Ceti - -	86.42. 6,1	−2,9	86.42. 7,3	−1,3	86.42. 9,8	+0,8	86.42.10,2	+1,2	86.42. 9,0
β Virginis -	87. 6.26,3	−2,0	87. 6.29,0	+0,7	87. 6.27,5	−0,8	not observed	—	87. 6.28,3
α Aquarii -	91.16.59,8	−4,3	91.17. 4,6	+0,5	91.17. 3,2	−0,9	91.17. 4,6	+0,5	91.17. 4,1
α Hydræ -	97.47.49,1	−3,0	97.47.48,8	−3,2	97.47.54,0	+2,0	97.47.53,0	+1,0	97.47.52,0
Rigel - -	98.26.28,8	−6,0	98.26.33,0	−1,8	98 26.34,5	−0,3	98.26.36,3	+1,5	98.26.34,8
Spica Virginis	100. 6.37,0	−3,8	100. 6.38,8	−2,0	100. 6.41,8	+1,0	100. 6.42,8	+2,0	100, 6.40,8
2 α Capricorni	103. 9. 3,2	−5,1	*103. 9.13,3	—	103. 9. 8,2	−0,1	103. 9. 7,8	−0,5	103. 9. 8,3
α Libræ -	105.11 55,6	−6,2	105.12.00,9	−0,9	105.12. 2,7	+0,9	not observed	—	105.12. 1,8
Sirius - -	106.26.56,3	−7.2	106.27. 4,1	+0,6	106.27. 4,0	+0,5	106.27. 1,8	−1,7	106.27. 3,5
Antares -	115.58.14,4	−9,7	115.58.24,3	—	115.58.24,0	—	not observed	—	115.58.24,1
Fomalhaut	120.40.30,9	−9,3	—	—	120.40.40,2	—	not observed	—	—
Polaris - -	1.45.34,5	—	1.45.34,5	—	1.45.36,2	—	1.45.37,0	—	1.45,36,0





XXII. *Observations and Remarks on the Figure, the Climate, and the Atmosphere of Saturn, and its Ring. By* William Herschel, *LL. D. F. R. S.*

Read June 26, 1806.

My last year's observations on the singular figure of Saturn having drawn the attention of astronomers to this subject, it may be easily supposed that a farther invesigation of it will be necessary. We see this planet in the course of its revolution round the sun in so many various aspects, that the change occasioned by the different situations in which it is viewed, as far as relates to the ring, has long ago been noticed; and Huygens has given us a very full explanation of the cause of these changes.*

As the axis of the planet's equator, as well as that of the ring, keeps its parallelism during the time of its revolution about the sun, it follows that the same change of situation, by which the ring is affected, must also produce similar alterations in the appearance of the planet; but since the shape of Saturn, though not strictly spherical, is very different from that of the ring, the changes occasioned by its different aspects will be so minute that only they can expect to perceive them who have been in the habit of seeing very small

* See *Systema Saturnium,* page 55, where the changes of the ring are represented by a plate.

objects, and are furnished with instruments that will show them distinctly, with a very high and luminous magnifying power.

If the equator of the planet Jupiter were inclined to the ecliptic like that of Saturn, I have no doubt but that we should see a considerable change in its figure during the time of a synodical revolution; notwithstanding the spheroidical figure occasioned by the rotation on its axis has not the extended flattening of the polar regions that I have remarked in Saturn. But since not only the position of the Saturnian equator is such that it brings on a periodical change in its aspect, amounting to more than 62 degrees in the course of each revolution, but that moreover in the shape of this planet there is an additional deviation from the usual spheroidical figure arising from the attraction of the ring, we may reasonably expect that our present telescopes will enable us to observe a visible alteration in its appearance, especially as our attention is now drawn to this circumstance.

In the year 1789 I ascertained the proportion of the equatorial to the polar diameter of Saturn to be 22,81 to 20,61,* and in this measure was undoubtedly included the effect of the ring on the figure of the planet, though its influence had not been investigated by direct observation. The rotation of the planet was determined afterwards by changes observed in the configuration of the belts, and proper figures to represent the different situation of the spots in these belts were delineated.† In drawing them it was understood that the shape of the planet was not the subject of my consideration, and that consequently a circular disk, which may be described

* Phil. Trans. for 1790, page 17. † Ibid. for 1792, page 22.

without trouble, would be sufficient to show the configurations of the changeable belts.

Those who compare these figures, and others I have occasionally given, in which the particular shape of the body of the planet was not intended to be represented, with the figure which is contained in my last Paper, of which the sole purpose was to express that figure, and wonder at the great difference, have probably not read the measures I have given of the equatorial and polar diameters of this planet; and as it may be some satisfaction to compare the appearance of Saturn in 1789 with the critical examination of it in 1805, I have now drawn them from the two papers which treat of the subject; Fig. 1, Plate XXI. represents the spheroidical form of the planet as observed in 1789, at which time the singularity of the shape since discovered was unknown; and Fig. 2 represents the same as it appeared the 5th of May, 1805. The equatorial and polar diameters that were established in 1789 are strictly preserved in both figures, and the last differs from the first only in having the flattening at the poles a little more extended on both sides towards the equatorial parts. It is in consequence of the increase of the length of this flattening, or from some other cause, that a somewhat greater curvature in the latitudes of 40 or 45 degrees north and south has taken place; and as these differences are very minute, it will not appear extraordinary that they should have been overlooked in 1789, when my attention was intirely taken up with an examination of the two principal diameters of the planet.

The use of various magnifying powers in observing minute objects is not generally understood. A low power, such as

200 or 160, with which I have seen the figure of Saturn, is not sufficient to show it to one who has not already seen it perfectly well with an adequate high power; an observer, therefore, who has not an instrument that will bear a very distinct magnifying power of 500, ought not to expect to see the outlines of Saturn so sharp and well defined as to have a right conception of its figure. The quintuple belt is generally a very good criterion; for if that cannot be seen the telescope is not sufficient for the purpose; but when we have intirely convinced ourselves of the reality of the phenomena I have pointed out, we may then gradually lower the power, in order to be assured that the great curvature of the eye-glasses giving these high powers, has not occasioned any deceptions in the figure to be investigated, and this was the only reason why I mentioned that I had also seen the remarkable figure of Saturn with low powers.

In very critical cases it becomes necessary to calculate every cause of an appearance that falls under the province of mathematical investigation. For this reason I have always looked upon an astronomical observation without a date as imperfect, and the journal-method of communicating them is undoubtedly what ought to be used. For instance, when it is known that my last year's most decisive observation, relating to the singular figure of Saturn, was made the 5th of May, astronomers may then calculate by this date the place of Saturn and of the earth; their distances from each other, and the angle of illumination of the Saturnian disk; by these means we find the gibbosity of the planet in the given situation, and ascertain that the defalcation of light could not then amount to the one hundredth part of a second of a

degree, and that consequently no error could arise from that cause.

I have divided the following observations into two heads, one relating intirely to the figure of the body of Saturn, the other concerning the physical condition or climate and atmosphere of the planet.

Observations of the Figure of Saturn.

In the collection of my observations on the planet Saturn, I have met with one made 18 years ago, which is perfectly applicable to the present subject, and is as follows:

August 2, 1788, 21^h $58'$. 20-feet reflector, power 300. Admitting the equatorial diameter of Saturn to lie in the direction of the ring, the planet is evidently flattened at the poles. I have often before, and again this evening, supposed the shape of Saturn not to be spheroidical, (like that of Mars and Jupiter,) but much flattened at the poles, and also a very little flattened at the equator, but this wants more exact observations.

April 16, 1806. I examined the figure of the body of Saturn with the 7 and 10-feet telescopes, but they acted very indifferently, and, were I to judge by present appearances, I should suppose the planet to have undergone a considerable change; should this be the case, it will then be necessary to trace out the cause of such alterations.

April 19. 10-feet, power 300. The polar regions are much flattened. The figure of the planet differs a little from what it appeared last year. This may be owing to the increased opening of the ring, which in four places obstructs now the

view of the curvature in a higher latitude than it did last year. The equatorial regions on the contrary are more exposed to view than they have been for some time past.

May 2. 10-feet, power 375. The polar regions are much flatter than the equatorial: the latter being more disengaged from the ring appear rather more curved than last year, so that the figure of the planet seems to have undergone some small alteration, which may be easily accounted for from our viewing it now in a different aspect.

The planet Jupiter not being visible, we cannot compare the figure of Saturn with it; but from memory I am quite certain that the flattening of the Saturnian polar regions is considerably more extended than those of Jupiter.

May 4. 10-feet, power 527. The equatorial region of Saturn appears to be a little more elevated than last year. This part of the Saturnian figure could not be examined so well then as it may at present, the ring interfering with our view of it in four places, which are now visible.

The flattening on both sides of the pole is continued to a greater extent than in a figure merely spheroidical, such as that of Jupiter; and this makes the planet more curved in high latitudes.

The planet being in the meridian, the equatorial shape of Saturn appears a little more curved than last year; but the air is not sufficiently pure to bear high powers well.

May 5. 10-feet, power 527. The air is very favourable, and I see the planet well with this power; its figure is very little different from what it was last year.

The polar regions are more extendedly flat than I suppose they would have been if the planet had received its form only

from the effect of the centrifugal force arising from its rotatory motion.

The equatorial region is a little more elevated than it appeared last year.

The diameter which intersects the equator in an angle of about 40 or 45 degrees is apparently a little longer than the equatorial, and the curvature is greatest in that latitude.

The planet being in the meridian and the night beautiful, I have had a complete view of its figure. It has undergone no change since last year, except what arises from its different situation, and a greater opening of the ring.

May 9. Power 527. The air being very clear, I see the figure of Saturn nearly the same as last year; the flattening at the poles appears at present somewhat less; the equatorial and other regions are still the same.

May 15, 10^h $30'$. I examined the appearance of Saturn, and compared it with the engraving representing its figure in last year's volume of the Phil. Trans. The outlines and all the other features of this engraving are far more distinct than we can ever see them in the telescope at one view; but it is the very intention of a copper-plate to collect together all that has been successfully discovered by repeated and occasional perfect glimpses, and to represent it united and distinctly to our inspection. Indeed by looking at the drawings contained in books of astronomy this will be found to be the case with them all.*

The equatorial diameter of my last year's figure is how-

* For an instance of this, see TOBIÆ MAYERI *Opera inedita. Appendix Observationum. Ad Tabulam Selenographicam Animadversiones,* where the annexed accurate and valuable plate represents the moon such as it never can be seen in a telescope.

ever a very little too short; it should have been to the polar diameter as 35,41 to 32, which is the proportion that was ascertained in 1789, from which I have hitherto found no reason to depart.

The following particulars remain as my last year's observations have established them.

The flattening at the poles of Saturn is more extensive than it is on the planet Jupiter. The curvature in high latitudes is also greater than on that planet. At the equator, on the contrary, the curvature is rather less than it is on Jupiter.

Upon the whole, therefore, the shape of the globe of Saturn is not such as a rotatory motion alone could have given it.

I see the quintuple belt, the division of the ring, a very narrow shadow of the ring across the body, and another broader shadow of the body upon the following part of the ring; and unless all these particulars are very distinctly visible we cannot expect that our instrument should show the outlines of the planet sufficiently well to perceive its peculiar formation.

May 16, 10^{h} 10′. The greatest curvature on the disk of Saturn seems to be in a latitude of about 40 degrees.

May 18. The difference between the equatorial and polar diameters appears to be a little less than the measures taken September 14, 1789, give it; but as the eye was then in the plane of the equator, and is now about 16 degrees elevated above it, we cannot expect to see it quite so much flattened at present.

June 3. The shadow of the ring falls upon the body of the ...rds of the ring, towards the limb; it grows a

little broader at both ends where it is upon the turn round the globe.

June 5. The planet Jupiter is not sufficiently high for distinct vision, and Saturn is already too low to use a proper magnifying power; but nevertheless the difference in the formation of the two planets is evident. The equatorial as well as polar regions on Jupiter are more curved than those of Saturn.

June 9. The air is beautifully clear, and proper for critical observations.

The breadth of the ring is to the space between the ring and the body of Saturn as about 5 to 4. See Fig. 3.

The ring appears to be sloping towards the body of the planet, and the inside edge of it is probably of a spherical or perhaps hyperbolical form.

The shadow of the ring on the planet is broader on both sides than in the middle; this is partly a consequence of the curvature of the ring which in the middle of its passage across the body hides more of the shadow in that place than at the sides.

The shadow of the body upon the ring is a little broader at the north than the south, so as not to be parallel with the outline of the body; nor is it so broad at the north as to become square with the direction of the ring.

The most northern dusky belt comes northwards on both sides as far as the middle of the breadth of the ring where it passes behind the body. It is curved towards the south in the middle.

I viewed Jupiter, and compared its figure with that of Saturn. An evident difference in the formation of the two

planets is visible. To distinguish the figure of Jupiter properly it may be called an ellipsoid, and that of Saturn a spheroid.

Observations on the periodical Changes of the Colour of the polar Regions of Saturn.

In the observations I have given on the planet Mars, it has been shown that an alternate periodical change takes place in the extent and brightness of the north and south polar spots;* and I have there suggested an idea that the cause of the brightness might be a vivid reflection of light from frozen regions, and that the reduction of the spots might be ascribed to their being exposed to the sun.

The following observations, I believe, will either lead us to similar conclusions with respect to the appearance of the polar regions of Saturn, or will at least draw the attention of future observers to a farther investigation of the subject.

With high magnifying powers the objects we observe require more light than when the power is lower; this affords us a good method of determining the relative brightness of the different parts of a planet. The less bright object will be found deficient in illumination when the power exceeds what it will bear with ease. I have availed myself of this assistance in the observations that follow.

June 25, 1781. With an aperture of 6,3 inches I used a magnifying power of 460. This gave a kind of yellowish colour to the planet Saturn, while the ring still retained its full white illumination.

November 11, 1793. From the quintuple belt towards the

* Phil. Trans. for 1784, page 260.

south pole the whole distance is of a pale whitish colour; less bright than the white belts, and much less bright than the ring.

This has been represented in a figure which was given in the volume of the Phil. Trans. for 1794, page 32. It is to be noticed that the south pole of the planet had been long exposed to the influence of the sun, and the former polar whitishness was no longer to be seen.

Jan. 1, 1794. The south polar regions are a little less bright than the equatorial belt.

Nov. 5, 1796. The space between the quintuple belt and the northesn part of the ring is of a bright white colour.

This seems to indicate that the whiteness of the northern hemisphere of Saturn increases when there is less illumination from the sun.

May 6, 1806. The north pole of Saturn being now exposed to the sun, its regions have lost much of their brightness; the space about the south pole has regained its former colour, and is brighter and whiter than the equatorial parts.

May 15. The south polar regions of Saturn are white; those of the north retain also some whitishness still.

May 18. With a magnifying power of 527, the south polar regions remain very white. The equatorial parts become of a yellowish tinge, and about the north pole there is still a faint dusky white colour to be seen.

June 3. The south polar regions are considerably brighter than those of the north.

These observations contrasted with those which were made when the south pole was in view complete nearly half a

Saturnian year, and the gradual change of the colour of the polar regions seems to be in a great measure ascertained. Should this be still more confirmed, there will then be some foundation for admitting these changes to be the consequence of an alteration of the temperature in the Saturnian climates. And if we do not ascribe the whiteness of the poles in their winter seasons immediately to frost or snow, we may at least attribute the different appearance to the greater suspension of vapours in clouds, which, it is well known, reflect more light than a clear atmosphere through which the opaque body of the planet is more visible. The regularity of the alternate changes at the poles ought however to be observed for at least two or three of the Saturnian years, and this, on account of their extraordinary length, can only be expected from the successive attention of astronomers.

On the Atmosphere of Saturn.

June 9, 1806. The brightness which remains on the north polar regions, is not uniform, but is here and there tinged with large dusky looking spaces of a cloudy atmospheric appearance.

From this and the foregoing observations on the change of the colour at the polar regions of Saturn arising most probably from a periodical alteration of temperature, we may infer the existence of a Saturnian atmosphere; as certainly we cannot ascribe such frequent changes to alterations of the surface of the planet itself: and if we add to this consideration the changes I have observed in the appearance of the

Fig. 1.

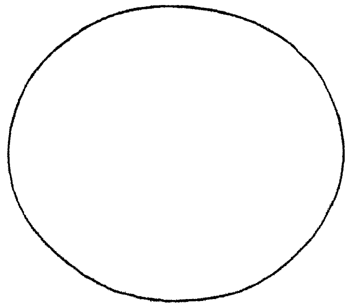


Fig. 2.

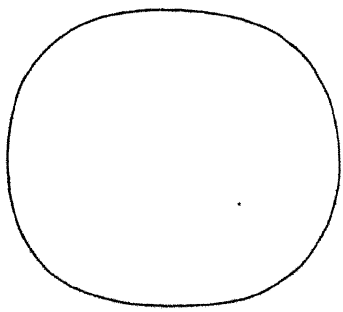
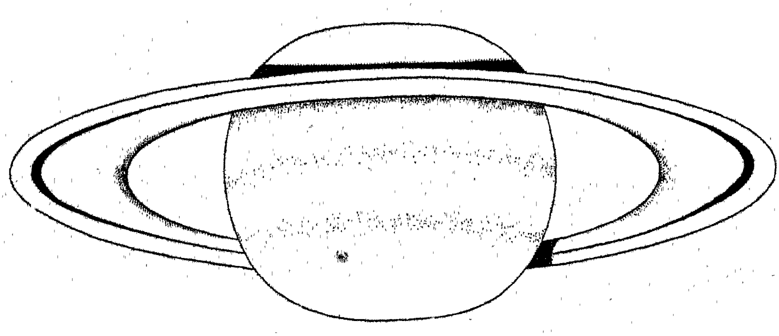


Fig. 3.



belts, or even the belts themselves, we can hardly require a greater confirmation of the existence of such an atmosphere.

A probability that the ring of Saturn has also its atmosphere has already been pointed out in a former Paper.

Slough, near Windsor,
June 12, 1806.

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	Journal de Chimie et de Physique, par J. B. van Mons. No. 17.	M. Van Mons, of Brussels.
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	The Philosophical Magazine, by A. Tilloch. No. 90.	Mr. Alexander Tilloch.
	The Critical Review, November, 1805.	The Editor.
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